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Education, Labor, and Health Disparities of Racial and Sexual Minorities

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Education, Labor, and Health Disparities of Racial and Sexual Minorities

by

Scott Michael Delhommer

Dissertation

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Dedicated to my parents, Nancy and Harold; my brother and sister-in-law, Rick and Susan; my niece, Cleo; and my girlfriend and partner, Morgan.

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Education, Labor, and Health Disparities of Racial and Sexual Minorities

by

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The three chapters of this dissertation explore the applied economics of inequality in educational attainment, labor market outcomes, and sexual health for racial and sexual minorities.

In the first chapter, I explore the role of same-race teachers reducing gaps in minority education, presenting the first evidence that matching high school students with same-race teachers improves the students' college outcomes. To address endogenous sorting of students and teachers, I use detailed Texas administrative data on classroom assignment, exploiting variation in student and teacher race within the same course, year, and school, eliminating 99% of observed same-race sorting. Race-matching raises minority students' course performance as well as improves longer-term outcomes like high school graduation, college enrollment, and major choice.

My second chapter examines how public policy can reduce labor market inequality across sexual orientation. I present the first quasi-experimental research examining the effect of both local and state anti-discrimination laws on sexual orientation on the labor supply and wages of lesbian, gay, and bisexual workers. To do so, I use American Community Survey data on household composition to infer sexual orientation and combine this with a unique panel dataset on local anti-discrimination laws. Using variation in law implementation across localities over time, I find these laws significantly reduce inequalities in the labor supply and wages across sexual orientation for both men and women.

The last chapter explores the moral hazard and health inequality implications of a life-saving HIV prevention drug, PrEP, for gay men. We document the first evidence of PrEP on aggregate STD and HIV infections. Using the pre-treatment variation in the gay male population, we show that male STD rates

were parallel in states with high and low gay population before the introduction of PrEP and begin to diverge afterwards. However, HIV infections were consistently downwardly trending before PrEP with no break at the introduction of PrEP, making inference of the effect of PrEP on HIV infections difficult. Specifically, we show that one additional male PrEP user increases male chlamydia infections by 0.55 cases, male gonorrhea infections by 0.61 cases, and male syphilis infections by 0.03 cases.

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Chapter I

High School Role Models and Minority College Achievement

1 Introduction

Educational outcomes have unequal distributions across race and income. Over 20 years, the test score gap between White and Black students decreased slightly but remains large with White students scoring 0.7 standard deviations higher on 4th grade standardized tests with similar findings for the White-Hispanic achievement gap (Reardon et al., 2013). While education may be described as a “great equalizer,” it can also contribute to and widen racial inequalities. Fryer Jr and Levitt (2004) show that Black and White students enter school with similar test scores in reading and math, but each year in school widens the gap by 0.1 standard deviations.

A number of potential explanations and mechanisms could be behind these differences including differing resources, differing qualities of schools, and others. Among these possible mechanisms is the racial distribution of teachers and how teachers improve outcomes for same-race students. Past literature shows that same-race teachers can also increase students’ short-term, course-level outcomes like test scores, grades, and behavior¹. Teachers may be more effective at communicating and teaching same-race students or may work effectively as role models, updating students’ beliefs about the returns to education or increasing their motivation². In support of the role model hypothesis, past literature shows that having a same-race teacher increases teachers’ and students’ expectations of student educational attainment³. Because race matching

¹(Dee (2004); Dee (2005); Fairlie, Hoffmann and Oreopoulos (2014); Egalite, Kisida and Winters (2015); Holt and Gershenson (2017); Lusher, Campbell and Carrell (2018))

²(Ladson-Billings (1995); Walker (2001); Marx and Roman (2002); Dee (2005); Gershenson et al. (2018))

³Ferguson (2003); Fox (2015); Papageorge, Gershenson and Kang (2018)

has been shown to significantly impact short-term outcomes like test scores and expectations, it is plausible that it can also impact longer-term outcomes. I explore the short-term and long-term effects from race matching using a fixed effect strategy paired with institutional knowledge to mitigate endogenous sorting of students and teachers.

An inherent issue in the student-teacher demographic matching literature is that students and teachers are not randomly assigned to each other⁴. Much of the literature focuses on elementary and middle school students because as students get older, they have a greater choice in their courses, increasing the risk of endogenous teacher selection (Paufler and Amrein-Beardsley, 2014). High school race matching has been largely unexamined because of the concerns of endogenous sorting despite high school's importance in college attendance and college major choice.

To address this endogeneity concern, I use newly collected Texas administrative data on classroom assignment and exploit institutional details of the assignment of students to teachers in Texas high schools. Texas is an extremely diverse state with large minority populations, making it an excellent setting to analyze racial matching of students and teachers. High school students in Texas have choices over courses, but they lack the choice of teacher conditional on courses. In order to minimize non-random sorting of students and teachers, I compare the outcomes of similar students in the same high school that selected the same courses but were assigned teachers of different races. My strategy reduces observable same-race sorting by about 99% for each race. To address concerns of remaining observable and unobservable endogenous sorting, I implement the bounding methodology proposed by Oster (2019) and show that the results for minority students are largely robust to the potential issue of selection on unobservable characteristics and omitted variable bias.

First, I examine the effect of matching with a same-race teacher in a course on course-level outcomes. Conditional on student and teacher fixed effects, I show that Black and Asian students perform significantly better on standardized test scores and have higher pass rates in the class that they match with a same-race teacher. These students score better along both objective and

⁴Clotfelter, Ladd and Vigdor (2006); Rivkin, Hanushek and Kain (2005); Paufler and Amrein-Beardsley (2014); Rothstein (2009); Koedel and Betts (2011)

subjective measurements of course performance with a same-race teacher, suggesting an increase in learning. I find no significant effect for short-term race matching effects for White or Hispanic students.

Next, I examine the longer-term effects of matching with a same-race teacher. I aggregate a student's 9th grade teachers and examine the effect of having an additional same-race teacher on the likelihood of graduating from high school and enrolling in college. To minimize non-random sorting, I compare students that selected the exact same set of courses but received teachers of different races. I find large effects from race matching for minority students in high school graduation and college enrollment. Hispanic and Asian students become significantly more likely to graduate high school with an additional same-race teacher. One additional race match for Black and Hispanic students significantly increases their likelihood of enrolling in college by 1 p.p. (2%) and 1.5 p.p. (3.4%), respectively. The effect for race matching for minority students is large in magnitude, but smaller than the only other estimate in literature. Gershenson et al. (2018) finds a 4 p.p. increase in college enrollment for Black students with one additional Black teacher in elementary school using random assignment, which is four times my estimate, suggesting my estimates are plausible. I find small, significantly positive effects for race matching for White students on two-year college enrollment, but I show that these results are not robust to the bounding methodology described in Oster (2019).

I also show that race matching in a subject in high school has strong effects for college major choice, especially for Science, Technology, Engineering, and Math (STEM) fields. One additional same-race STEM teacher in high school for Black and Hispanic students increases a student's likelihood of majoring in STEM as a college freshman by 0.7 p.p. (8.6%) and 0.6 p.p. (6.6%), respectively. An additional Hispanic teacher in social sciences for a Hispanic student increases their likelihood of majoring in social sciences by 0.3 p.p. When examining major choice one concerning pattern is that Black and Hispanic students in college are less likely to major in STEM fields, which have larger wage premiums (Altonji, Blom and Meghir, 2012). These differences in major choice could contribute to racial income gaps. My findings paired with the dearth of Black

and Hispanic students in STEM fields suggest a need to train and hire more STEM minority teachers.

I find large effects for race-matching on longer-term outcomes like college enrollment, but that specification imposes linearity, which could be a strong assumption. How the marginal effect of race-matching varies with the number of matches is an important policy relevant question as well. I relax this linearity assumption and explore potential non-linear effects of race matching on these longer-term, individual level outcomes. The effect of race matching for Black and Hispanic students plateaus after two race matches for certain outcomes like two-year college enrollment, suggesting that the effect of race matching could be non-linear. In the non-linear model, White students do not significantly benefit from any race matches in any outcomes, giving further evidence that these race-match effects are strongest for minority students. (Gershenson et al., 2018) developed a model using marginal returns to race matching to explore mechanisms where constant marginal returns imply that teachers are more effective at teaching same-race students while diminishing marginal returns imply that teachers act as role models, updating students' beliefs about the returns to education. My evidence provides further evidence that role model effects may be the mechanism that same-race teachers improve student outcomes.

While I find large effects for minority students, I do not find robust, significant evidence for White students benefiting from having same-race teachers on short-term, course-level outcomes like test scores nor on longer-term, individual-level outcomes like college enrollment. White teachers make up over 70% of high school teachers in Texas while White students only make up about 33% of the student population. My research suggests that policies to make the teaching population more representative would likely benefit minority students with minimal negative spillovers impacting the White student population from having fewer same-race teachers.

To test how educational attainment would change with more minority teachers, I conduct the following thought experiment using Monte Carlo simulations: suppose that the racial distribution of the teachers matches the racial distribution of the students. In 5,000 simulations, I randomize teacher race with weights such that each high school has a racial distribution of teachers that is representative of the racial distribution of students. This thought experiment is likely an infeasible

ble policy, but it still may be instructive to examine how a more representative teacher population would affect students. Next, I see how many additional or fewer race matches each race of students receives and then multiply by my point estimates to get back-of-the-envelope calculations. I find large increases in Hispanic high school graduation and college enrollment and relatively small decreases for White students. Specifically, my back-of-the-envelope calculations suggest that a representative teaching population would increase the aggregate high school graduation rate by 0.5 p.p. or about 2,500 additional students graduating high school and would increase the aggregate college enrollment rate by 0.8 p.p. or about 4,000 additional students enrolling in college.

I contribute to the previous literature in three main ways. First, I present the first research examining the effect of race matches in high school on college outcomes such as enrollment and major choice. High school race matching has gone largely unexamined despite the importance of high school in college enrollment and major choice⁵. These long-term effects of race matching on high school graduation and college enrollment have also gone unexamined with the exception of Gershenson et al. (2018), which examines the effect of race matching for Black and White elementary students. They omit Hispanic and Asian students from their analysis for sample sizes issues. However, Hispanic and Asian students grow every year as a percentage of the population, and together make up over 50% of the student population in Texas. My second contribution is that I include Hispanic and Asian students in my analysis, presenting the first evidence on the effect of race matching at any school level for Hispanic and Asian students on longer-term outcomes such as high school graduation and college enrollment. Finally, I add to the literature by providing additional estimates of the short-term effect of matching with a same-race teacher on test scores and pass rates using a large, detailed administrative dataset.

⁵(Maltese and Tai (2011); Rask (2010))

2 Literature Review

Examining the effect of student-teacher demographic matching is not new. However, there is a dearth of literature examining high school race matching. An inherent issue in the student teacher matching literature is that students and teachers are not randomly assigned to each other, except in special cases like the STAR classroom experiment in Tennessee or in the military⁶. Typically, researchers believe that this non-random sorting of students and teachers increases with student age from elementary school to middle school to high school as students have greater choices in courses (Paufler and Amrein-Beardsley, 2014). Hence, researchers focus their attention on elementary and middle school where endogenous sorting is less likely to occur, leaving high school race matching woefully underexamined despite being an important time in a student's educational career.

I contribute to the literature by presenting the first evidence examining the effect of high school race matching on college outcomes. Sass (2015) is the only other paper to examine the interaction of high school teacher race with student race. However, he only examines indirect matching, using high school faculty composition and cannot directly observe matching within a classroom. While this method is instructive, teacher composition may impact students through non-classroom interactions such as extracurricular activities like sports and clubs as well as having potential spillovers to affecting other teachers through peer effects. Estimating the effect of race matching using the faculty composition will provide a muddled estimate of the true effect of matching in a classroom. By observing the direct match of students and teachers within a section in a course will allow me to better estimate the effect of race matching.

This research also fits in with a few different literatures in the education field. It most closely relates to the student-teacher demographic matching literature. I will draw a distinction in the types of outcomes examined in the literature between short-term and longer-term outcomes. Short-term outcomes are outcomes that change in the classroom when a student and teacher match with each other like grades, behavior, expectations, and test scores. Long-term outcomes are outcomes that are realized years after a race match like college enrollment or graduation.

⁶(Dee (2005); Carrell, Page and West (2010); Gershenson et al. (2018))

Much of the early research shows there are short-term benefits to race matching⁷. Much of the literature on sex matching complements this work and suggests similar effects for sex matching⁸. The literature suggests positive effects overall from demographic matching, but there is a notable hole in the literature on short-term benefits. These studies typically focus exclusively on White and Black students with only one other study examining course outcomes from race matching for Hispanic and Asian students (Egalite, Kisida and Winters, 2015). I contribute to the literature by providing the only other estimates of short-term benefits from race matching for Hispanic and Asian students as well as providing additional estimates for White and Black students using a large administrative dataset.

Most of the student-teacher race matching literature focuses on short-term outcomes, missing the longer-term effect of race matching. I present the first paper to examine long-term effects of race matching for Hispanic and Asian students. Only one paper tackles the question of long-term effects by looking at the Tennessee STAR randomization experiment (Gershenson et al., 2018). They show that Black students that were randomly assigned a Black teacher in Kindergarten to third grade are 5 p.p. more likely to graduate high school and 4 p.p. more likely to enroll in college. However, they fail to address how race matching affects Hispanic and Asian students, which I examine. Many of the race matching studies focus exclusively on Black and White students for sample size purposes⁹. However, Hispanic students are nearly 50% of public high school students in Texas. The Hispanic and Asian population are large and growing groups in the United States, underlying the importance of their inclusion.

Gershenson et al. (2018) examines the long-term effects of race-matching for Black and White students in elementary school on high school graduation and college enrollment. My paper furthers the literature by analyzing the timing of race-matching and its importance. Heckman emphasizes the need for interventions to occur as early in a childhood as possible, showing that effects impacting skills and learning grow and build over time in a synergistic fashion (Heckman (2000); Cunha and Heckman (2007)). Race-matching in elementary school has large effects on

⁷(Dee (2004); Dee (2005); Fairlie, Hoffmann and Oreopoulos (2014); Egalite, Kisida and Winters (2015); Holt and Gershenson (2017); Lusher, Campbell and Carrell (2018))

⁸(Nixon and Robinson (1999); Dee (2006), Dee (2007); Cho (2012); Winters et al. (2013); Sansone (2017))

⁹(Dee (2004); Dee (2004); Gershenson et al. (2018))

high school graduation and college enrollment (Gershenson et al., 2018). Whether or not this effect is still present from race-matching in high school is an open question and informs this broader literature on the timing of educational interventions. Students exposed to same-race teachers in high school may not have sufficient time to change their skills and human capital from the treatment relative to students in elementary school. Students in high school may also have stickier beliefs about returns to education, suggesting an attenuated effect relative to an elementary school match. I confirm this hypothesis in the results section, showing that the effect of Black students matching in high school on college enrollment is about 25% smaller than what Gershenson et al. (2018) finds for matching in elementary school.

There are several potential reasons why demographic matching of students and teachers may improve student outcomes. One theory of race matching impacting student outcomes is that same race teachers have a culturally relevant pedagogy (Ladson-Billings, 1995). Students benefit from same-race teachers because those teachers can effectively communicate in a cultural context by helping students affirm their cultural identity and challenge inequities that schools can instill and perpetuate (Ladson-Billings, 1995). Related to culturally relevant pedagogy, cultural synchronization describes the interpersonal cultural context that exist between Black students and teachers (Irvine, 1990). This cultural relevancy of same-race teachers has also been documented qualitatively in Native Hawaiian children and native American students in observing student-teacher interactions (Au and Jordan (1981); Mohatt, Erickson et al. (1981)).

Another potential reason is that underrepresented students interacting with a same-race teacher may benefit from a role model effect, updating their beliefs about returns to education and increasing their learning (Walker (2001); Marx and Roman (2002); Dee (2005)). A growing literature on students' and teachers' beliefs and expectations suggest that race-matching can greatly improve a student's expectations about their own abilities, an important input to education (Dee (2005), Fox (2015); Papageorge, Gershenson and Kang (2018)).

This research also informs the college major choice literature. Much of the major choice literature focuses on college interventions like faculty composition or peer groups ¹⁰. Some of

¹⁰(Canes and Rosen (1995); Bettinger and Long (2004); Ost (2010); Price (2010); Fischer (2017))

this literature focuses on the persistence of students in a STEM major, but it misses a crucial determinant of major preference – high school. Most of the work on high school factors impacting major choice in college are descriptive but suggest that high school is where many students form their preferences for subjects and subsequently their future major in college ¹¹. The best paper on high school determinants of college major choice instrumented for additional math and science course taken with state law changes and show a significant increase in STEM majors (Federman, 2007).

In conclusion, I contribute to the literature in three main ways. First, I am the first to examine direct high school race matches on college outcomes such as enrollment and college major choice. Second, I am also the first to examine long-term effects for race matching for Hispanic and Asian students. Finally, I add to the literature by providing additional estimates of the short-term effect of matching with a same-race teacher on test scores and pass rates using a large, detailed administrative dataset. Texas, the setting of my research, is an incredibly large and diverse state with many Hispanic, Black, Asian, and White students allowing me to take a robust snapshot of race matching in high school.

3 Data

Texas is an excellent state to examine the effects of race matching because it is a large and diverse state. My data come from the Texas Education Research Center, combining data from the Texas Education Agency (TEA), the Texas Higher Education Coordinating Board (THECB), and National Student Clearinghouse (NSC). The TEA, the regulatory body that oversees K-12 education in Texas, collects data on student enrollment, demographics, and outcomes on the universe of students in public K-12 education. The THECB is the higher education counterpart to TEA. The THECB only collects information on students in higher education in Texas, so I supplement that data with NSC data, which contains enrollment and major choice for all colleges in the United

¹¹(Maltese and Tai (2011); Rask (2010); Morgan, Gelbgiser and Weeden (2013); Bottia et al. (2015); Bottia et al. (2018))

States. The datasets are linked using de-identified Social Security numbers, ensuring high-quality links between high school and college outcomes.

I observe every section that a student is assigned to in every course that student takes. I also observe every student and every teacher in every public high school from 2012 to 2016. The data differentiate between Advanced Placement and International Baccalaureate courses and regular courses as well as advanced and remedial courses, allowing me to control for different tracking of students. I follow the 2012 and 2013 freshmen cohorts for four years in high school and one year afterwards to examine college enrollment and major choice. I limit my course sample to sections without co-teachers that have section sizes larger than or equal to five and less than or equal to 40. Sections smaller than 5 are typically special classes with selected students. Most sections larger than 40 are mainly large physical education courses. Finally, I eliminate all music, art, physical education, and foreign language courses to focus on academic courses.

Starting in 2012, TEA began collecting class-level data on which teachers were assigned to which student in a given course and whether a student passed that course or not. I link the course level information to student test scores, which TEA administers in five courses: Algebra I, Biology, English I, English II, and US History. I normalize the test scores to a Z-score with a mean of zero and standard deviation of one. The TEA administers these standardized tests to all students that take these courses in the state, and the students must pass these tests to receive credit for the courses. These standardized tests are high-stakes, multiple choice tests with little to no room for subjective grading. These are the only five standardized tests that are administered to students in high school in Texas, so I can only examine the effect of race matching on a student's test scores in these five courses. Another outcome I examine is whether the student passed their course, which I observe in every course. These two related yet distinct measures of course performance allows for testing of different mechanisms. The state of Texas grades test scores objectively through multiple choice grading, while teachers determine if a student passes a class through some combination of grading of assignments and interactions in the class.

My outcomes separate into two different categories: short-term, course-level outcomes, and long-term, individual-level outcomes. The short-term, course-level outcomes such as test Z-

scores and an indicator for passing a course vary at the course level where I can see a student have multiple outcomes. I examine the effect of race matching in a course on these outcomes which are vary at the course-level. The long-term outcomes such as high school graduation, college enrollment, and major choice are individual-level outcomes that are constant for a given individual. I estimate the effect of race matching at the individual-level after a student matches with a 9th grade teacher of the same race. I define high school graduation as graduating four years from 9th grade and enrollment in college as enrolling five years from 9th grade. I observe what major students choose as a freshman in college and if they attend a two- or four-year college. I examine all academic courses that every student in public high school takes and the teacher that they are assigned, resulting in about 9 million student-course observations for 649,320 students and 78,453 teachers.

Another benefit to using the Texas administrative data is the inclusion of detailed demographic information. TEA data also contain information on students including race, age, sex, free or reduced-price lunch status and indicators for if a student is considered gifted or at-risk of dropping out. I also include 8th grade test scores taken by students to control for pre-high school ability in school. The data contains detailed demographic information on teachers as well as students, including race, sex, education level, pay, tenure, experience, and age. I use the detailed demographics of students and teachers to determine race matches and control for potentially confounding factors.

I present some descriptive statistics on differences in academic outcomes and characteristics by race in Table I.1 where the unit of analysis is the student in Panel A. Unsurprisingly, large gaps exist in academic achievement along racial lines. Black and Hispanic students are far less likely to graduate high school, enroll in college, and major in STEM compared to their Asian and White counterparts. They are also less likely to pass their courses and on average have lower standardized test scores. Asian students have the highest rates of academic achievement with respect to high school graduation, college enrollment, test scores, and pass rates. Students also show extreme disparities in student characteristics along race lines that may be a function of a lack of representation in teachers at a younger age in elementary school. For instance, Black and

Hispanic students are far less likely to be considered “gifted and talented” than White or Asian students.

These racial gaps in academic achievement shown in Panel A could be attributable in part to the racial distribution of teachers. In Panel B of Table I.1 I show the statewide racial composition of students and teachers. The teacher composition in high school is overwhelmingly White relative to the student population. White students make up 33% of the student population but over 70% of the teachers in the sample are White. Black and Hispanic teachers comprise only about 9% and 18% of the sample, respectively. Black, Hispanic, and Asian students are underrepresented in the teacher composition relative to White teachers who comprise the vast majority of teachers.

4 Methodology and Empirical Strategy: Short-term

4.1 Sorting

An inherent issue in the student-teacher matching literature is that schools do not randomly assign students to teachers. Unfortunately for the econometrician, students have choices over what courses to take and subsequently what teachers they are assigned. Researchers commonly believe that the non-random sorting of students and teachers increases as students’ age increases from elementary to middle school and middle school to high school as students get older and are given greater freedom in which courses they can take (Paufler and Amrein-Beardsley, 2014). This belief about sorting helps explain why much of the student-teacher matching literature focuses on elementary and middle school students with a dearth of studies on high school matching.

Sorting, especially along race lines, is most prominent across high schools with large amounts of racial segregation across schools. However, sorting also occurs within schools as well (Clotfelter, Ladd and Vigdor (2006); Rivkin, Hanushek and Kain (2005); Paufler and Amrein-Beardsley (2014); Rothstein (2009); Koedel and Betts (2011)). The pre-college literature on matching in observational studies focuses on using student fixed effects (Dee (2007); Winters et al. (2013); Sansone (2017)), but researchers admit that student fixed effects are likely not sufficient to solve sorting as an issue (Dee, 2005).

Another attempt to get around non-random sorting is to use variation in teacher composition in college faculty to instrument for a match by sex (Bettinger and Long, 2004). The technique has been applied in matching papers for race in elementary school (Gershenson et al., 2018). One potential issue is that these papers do not identify the effect of a match but instead are identifying off of a general composition change, and the effect could be working through other mechanisms such as extracurricular activities with student clubs and athletics (Gershenson et al., 2018).

Conversations with Texas high school administrators suggest that students have choices over the courses they take, but they have no choice in which teacher they are assigned conditional on course selection. The precise mechanism for assigning students and teachers to classroom likely varies across school districts and schools and is unobservable. The natural experiment that I will exploit is by comparing two students in a given high school that both take the same course in same academic year, but one student is assigned teacher A and the other is assigned teacher B. For example, two Black students in the same high school choose to both take Algebra 1 in 2013, but one student is assigned to Black teacher and the other is assigned to a White teacher. To implement this, I employ a high school-by-year-by-course fixed effect.

My identifying variation in this case will be courses in a given high school and year that have multiple teachers with different races such that one teacher would produce a race match for a student and the other teacher would not. I present the high school campuses with courses that have multiple teachers with different races in Figure I.1, showing the relationship between students and teachers of the same race. The gray points in the scatter plot show which schools lack multiple different race teachers for any courses. The identifying variation comes from the courses at the high schools with color. The figure shows a strong clear correlation between the student and teacher composition at the school level, stressing the need to examine within a high school. It also shows that the campuses that are heavily composed of one race for teachers do not contribute to the identifying variation since they lack multiple race teachers at the course level. The figure shows that there are several points close to each other but some campuses have courses contributing to the identifying variation and others do not. Campuses with similar student and

faculty composition can also vary in whether they have courses with multiple teachers of different races.

One way to test my fixed effect strategy is to examine how likely a student of a given race/ethnicity is to be assigned a teacher of the same race/ethnicity and how that likelihood changes as I introduce additional information. In an ideal scenario, one would randomize students to teachers, and students would be no more likely to receive a same-race teacher than a student of a different race. For example, a Black student would ideally be no more likely than a White student to have a Black teacher. To test how similar the student to teacher assignment is to the ideal experiment, I run the following regressions with varying fixed effects to see how sorting changes where the unit of analysis is at the student by course level.

$$BlackTeach_{jc} = \alpha_1 BlackStu_{ic} + \alpha_2 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.1)$$

$$HispTeach_{jc} = \beta_1 HispStu_{ic} + \beta_2 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.2)$$

$$AsianTeach_{jc} = \gamma_1 AsianStu_{ic} + \gamma_2 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.3)$$

$$WhiteTeach_{jc} = \delta_1 WhiteStu_{ic} + \delta_2 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.4)$$

In these equations, I regress an indicator for student i 's race in course c on an indicator for teacher j 's race. I include a vector of student characteristics including demographic and socioeconomic factors such as free and reduced priced lunch status, gifted and talented status, and 8th grade test scores to control for pre-high school ability in school. Finally, I include ψ_{hct} giving high school by year by course fixed effects. The coefficients of interest are α_1 , β_1 , γ_1 , and δ_1 . In equation I.1, α_1 represents the likelihood that a Black student will have a Black teacher relative to a White student as White students are used as the reference group. β_1 and γ_1 represent the likelihood a Hispanic or Asian student will have a same-race teacher relative to a White student.

Finally, δ_1 represents the likelihood that a White student will have a White teacher relative to a Hispanic student.

In Figure I.2, I show how α_1 , β_1 , γ_1 , and δ_1 vary, becoming closer to zero when changing from no fixed effect to high school fixed effect to high school-by-year-by-course fixed effects. I cluster my standard errors at the school level. I present the full output in regression tables in the Appendix Tables A1-A4. We can see that in the regression without fixed effects, Hispanic students are about 22 p.p. more likely to have a Hispanic teacher relative to a White student. However, when using the high school by year by course fixed effects, this non-random sorting is greatly reduced to Hispanic students being 0.12 p.p. more likely to have a Hispanic teacher, a more than 99% reduction in the likelihood. While Hispanic students are statistically significantly more likely to have a Hispanic teacher, the increased likelihood is economically small. The increased likelihood suggests that for every 820 courses that Hispanic students take, there is one additional Hispanic teacher than there would be under true random assignment.

Another way to empirically test how sorting changes is to examine which covariates of students predict having a same race teacher and how those covariates change with additional fixed effects. In an ideal experiment with randomization, Hispanic students receiving free or reduced priced lunch would be no more likely to receive a Hispanic teacher than Hispanic students paying full price for lunch. I limit each regression to one race of students and see which covariates predict same race teachers. Specifically, I run the below regressions, limiting each regression to only one race to examine what covariates of Black students predict having a Black teacher or what covariates of Hispanic students predict having a Hispanic teacher.

$$BlackTeach_{jc} = \alpha_3 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.5)$$

$$\text{if } StuBlack_{ic} = 1$$

$$HispTeach_{jc} = \beta_3 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.6)$$

$$\text{if } StuHisp_{ic} = 1$$

$$AsianTeach_{jc} = \gamma_3 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.7)$$

$$\text{if } StuAsian_{ic} = 1$$

$$WhiteTeach_{jc} = \delta_3 X_{it} + \phi_{hct} + \epsilon_{ijhct} \quad (I.8)$$

$$\text{if } StuWhite_{ic} = 1$$

The coefficient α_3 , represents the likelihood that a Black student with a given characteristic will have a black teacher, and β_3 represents the likelihood that a Hispanic student with a given characteristic will have a Hispanic teacher. I cluster my standard errors at the school level. I present these coefficients and how they change in response to different levels of fixed effects in Figure I.3. The corresponding regression tables are displayed in Appendix Tables A5-A8.

A consistent pattern of convergence to zero seen in the two sorting methodologies suggests that including high school fixed effects better controls for non-random sorting than the regressions without fixed effects, as seen in Sass (2015). Including the course information by including the high school by year by course fixed effects also reduces the non-random sorting further than the high school fixed effects as well.

4.2 Estimation

I examine two different types of outcomes: course-level outcomes and individual-level outcomes. The course-level outcomes such as test Z-score and pass rates vary at the course for each individual. This variation allows me to estimate the effect of race matching with the inclusion of student-level fixed effects to control for time-invariant student characteristics. Including student fixed effects is another strategy that previous researchers have used to control for non-random, endogenous sorting of students and teachers.¹² I also include the high school by year by course

¹²Dee (2007); Winters et al. (2013); Sansone (2017)

fixed effect to minimize sorting. For the course-level outcomes, I estimate the effect of race matches using this strategy below where the unit analysis is at the student by course level:

$$Z_{ijct} = \beta_1 BlackMatch_{ijc} + \beta_2 HispMatch_{ijc} + \beta_3 AsianMatch_{ijc} + \beta_4 WhiteMatch_{ijc} + \gamma_1 X_{it} + \gamma_2 \pi_{jt} + \psi_i + \delta_j + \phi_{hct} + \epsilon_{ijhct} \quad (I.9)$$

Here Z_{ijct} gives the Z-score for a standardized test for student i with teacher j in course c in year t . Another outcome examined is an indicator for if the student passes her course. Each match variable is an indicator for if the student matches along race lines with her teacher in course c . There is a vector for time-varying student characteristics in X_{it} and for time-varying teacher characteristics in π_{jt} . Finally, I include three fixed effects: student fixed effects, ψ_i , which controls time-invariant unobservable student characteristics; teacher fixed effects, δ_j , to control for time-invariant teacher characteristics like quality; and high school by year by course fixed effects, to control for non-random sorting of students and teachers across courses. I cluster my standard errors at the school level.

The coefficients of interests are β_1 , β_2 , β_3 , and β_4 and the interpretation on the coefficients is the effect of having a race match for Black, Hispanic, Asian, and White students, respectively. These coefficients estimate the effect of a race match relative to another student of that same race who received a different race-teacher. For instance, β_1 gives the effect for a Black student having a Black teacher relative to a Black student having a non-Black teacher conditional on student and teacher fixed effects.

This estimation strategy is akin to a difference-in-difference with the high school by year by course and student fixed effects. The student fixed effects make the regression estimates the change in a student's test scores with a same-race teacher relative to that student's test scores with a different-race teacher. The high school by year by course fixed effect will compare students with a race match to students without a race match taking the same course. The result of having both in the equation is that β_1 , β_2 , β_3 , and β_4 are estimating the change in a student's test scores with a race match to the change in a student's test score without a race match.

For example, without the student fixed effect, β_1 would compare the test score of a Black student with a Black teacher to the test score of a Black student with a non-Black teacher, which is

one difference. Adding in the student fixed effect will make the comparison within the student as well, adding another difference. Using both student and high school by year by course fixed effect creates two differences that contribute to the coefficients of interest. This estimation strategy is not a traditional difference-in-difference estimation, but it is a similar strategy by comparing the difference between the change in test scores for students with and without a same-race teacher.

5 Short-term Results

5.1 Course-level Outcomes

In Table I.2, I present the course-level outcomes and the effect of a race match. When examining the short-term outcomes from the course that a student matches with same-race teacher, Black and Asian students perform significantly better. Black students matched with Black teachers perform 0.013 standard deviations better on standardized tests and are 0.9 p.p. more likely to pass their courses than they would have if they had a non-Black teacher. Asian students have a larger premium from race matching with an increase of 0.07 standard deviations on standardized tests and become 1.4 p.p. more likely to pass their courses. Test scores are limited to only five courses: Algebra I, Biology, English I, English II, and U.S. History, so the sample size is smaller than that of the regression looking at the course pass rate.

Ultimately, test scores and pass rates are two different measures of a student's performance in a course. However, they vary in how they are graded. Texas administers the tests to all students in the state that take Algebra I, Biology, English I, English II, or U.S. History. The tests are graded externally and are multiple choice, eliminating subjective grading. The English exams have essays, which are also graded by the state with a systematic rubric. Students must pass these high-stakes tests to earn credit for the courses, each of which is a mandated class to graduate high school. On the other hand, passing a class is dependent on the student-teacher interactions with a teacher's discretion playing a role at multiple points. Whether or not a student passes a class is more a subjective measure than the student's performance on a standardized, state-wide exam. One concern would be if students became more likely to pass their classes but

did not improve their test scores, which could indicate that teachers are more favorable with their grading to same-race students. However, given that there are significant improvements in test scores and pass rates, the findings suggest that Black and Asian students perform better with same-race teachers in objective and subjective measures of performance.

Interestingly, only Black and Asian benefit from race matching in the short-term. Hispanic and White students do not significantly improve their test scores or pass rates. To assess reasons for why there may be an effect for Black and Asian students, we should assess the mechanisms for an improved performance with a same-race teacher. There are two main theories behind why students perform better with same race teacher. The first is that teachers more effectively communicate ideas and teach same-race students because there is some shared cultural connection (Irvine (1990); Ladson-Billings (1995)). The second is that teachers act as role models to students, updating their beliefs about own abilities or returns to education, and these role models may be more effective for students who are more underrepresented (Marx and Roman (2002); Dee (2005)).

One potential explanation for this split in race-match results is that Black and Asian teachers and students are relatively rare compared to White and Hispanic teachers and students in Texas. As I show in Table I.1, Black and Asian teachers make up 9% and 2% of the teacher population, and Black and Asian students make up a small fraction of the student body in Texas high schools at 14% and 4%, respectively. Making up a smaller proportion of the student body may make race-matching more salient for Black and Asian students. In particular, it may mean that the culturally relevant interactions for Black and Asian students that are matched with a same-race teacher are more poignant and more effective, whereas Hispanic and White students are effectively a majority already at 50% and 33% of the student population, respectively. However, these culturally relevant interactions for Black and Asian students do not necessarily preclude role model effects from occurring either as both of these mechanisms could be working together. Later on, I will test whether or not role model effects are a potential mechanism for long-run outcomes using a model developed by Gershenson et al. (2018).

5.2 Heterogeneity

One potential source of heterogeneity is in student ability. It is possible that the gains from student-teacher race matching are stronger for students at the bottom of the distribution. Given that Black and Asian students are more likely to pass their classes when they have a same-race teacher would suggest that students at the bottom of the distribution are affected because a student going from a “C” to an “A” would not be captured by the passing measure, but a student going from a “F” to a “C” would be captured in this measure.

To explore how matching affects students of different ability, I break down the sample into quartiles by 8th grade standardized test scores in reading and re-run the analysis. Given the tests were taken in 8th grade, they will be unaffected by any intervention at the high school level and should work as an approximation for a student’s underlying ability in school. I present the course-level outcomes in Table I.3.

The pattern seen in the average effects is mostly the same with Black and Asian students benefiting from matching via race lines with one exception. Hispanic students at the bottom of the test score distribution become 0.3 p.p. more likely to pass their classes with a Hispanic teacher. For Black students, the effect of race matching on test scores and pass rates is negatively correlated with 8th grade reading scores. There is also a negative correlation in the effect size of race matching on tests score, but no correlation for the effect on passing a course.

The correlations between 8th grade reading scores and the effect of race-matching on improving test scores suggest that Black and Asian students at the lower-end of the ability distribution are most impacted by having a same-race teacher. It is possible that the lower-ability students can make up more ground from a race-match relative to their higher-ability peers. There is a different interpretation for the effect on pass rates though. The declining effect may not be a function of decreased effectiveness of a match. A student at the higher end of the ability distribution may also benefit from matching but the passing rate may not be the margin that is impacted. A high-ability student may go from earning a “B” in a class to earning an “A” from race matching with a teacher, but that would not impact their pass rate. However, high-ability Black and Asian

students still have their pass rate significantly improved from race matching, even though these students were already more likely to pass their classes than their lower-ability peers.

6 Methodology and Empirical Strategy: Long-term

6.1 Sorting

In the short-term empirical strategy, my previous unit of analysis is at the student by course level, which naturally fit with course-level outcomes like test scores and pass rates. However, for longer-term, individual-level outcomes like high school graduation, college enrollment, and major choice that vary at the individual level the unit of analysis should be at the student level. My identification strategy for course-level outcomes comes from variation at the course-level with respect to which teachers are assigned to which students and uses a high school by year by course fixed effect. Aggregating the data to the student-level will lose this granular course information. I implement a course-set fixed effect to circumvent this aggregation problem. The course-set fixed effect will group together and compare student who took the exact same set of academic courses in the 9th grade in a given high school, allowing the aggregated student-level outcomes to retain course information.

This course-set fixed effect effectively exploits the same variation as the high school-by-year-by-course fixed effect while also controlling for the other courses that a student also took. I compare students in the same high school with identical course selections, but one student idiosyncratically receives more same-race teachers than a different student. For example, two Black freshmen students at the same high school both selected to take Algebra 1, English 1, Chemistry, and Geography, but one of these students received a Black Algebra 1 teacher and the other received a White Algebra 1 teacher. My strategy will compare these two students because they have the same course-set but different teachers conditional on their course selection.

My sample selection changes slightly as there are some students without a comparable student with an identical course-set in a given high school, but my sample is still left with over 500,000 students over two cohorts. I present summary statistics in Table I.4, which suggest that the new

sample is slightly negatively selected with respect to high school graduation as compared to the previous sample with all students in it. However, the racial composition of students and teachers looks nearly identical to the sample in the first section examining short-term outcomes. The new sample is similar in enrollment to the old sample as well.

I create the course-set using the 9th grade courses that a student selects and the teachers they are as. I focus on 9th grade courses because down-stream race matches in later grades are potentially endogenous. Race matching in the 9th grade potentially affects a student's propensity to remain in high school. Later race matches in the 10th, 11th, and 12th grade are a function of race matching in the 9th grade and are endogenous if 9th grade race matches increase a student's likelihood to persist in school. However, I do show in the robustness check section that including 10th grade courses does not meaningfully change the estimates.

I run similar sorting estimation strategies as the previous section to see how well this specification controls for endogenous sorting. Similar to the previous sorting strategies, I present the regression output with varying levels of fixed effects to examine how non-random sorting changes and gets closer to zero using the full course-set fixed effects. Specifically, I estimate the below equations:

$$NumBlackTeach_i = \alpha_1 BlackStu_i + \alpha_2 X_i + \kappa_s + \epsilon_{is} \quad (I.10)$$

$$NumHispTeach_i = \beta_1 HispStu_i + \beta_2 X_i + \kappa_s + \epsilon_{is} \quad (I.11)$$

$$NumAsianTeach_i = \gamma_1 AsianStu_i + \gamma_2 X_i + \kappa_s + \epsilon_{is} \quad (I.12)$$

$$NumWhiteTeach_i = \delta_1 WhiteStu_i + \delta_2 X_i + \kappa_s + \epsilon_{is} \quad (I.13)$$

In these equations, I regress the number of same-race teachers that a student has in 9th grade on an indicator for a student's race on. X_i is a vector of student characteristics includ-

ing gifted/talented status, free/reduced price lunch status, and 8th grade test scores. κ_s is the course-set fixed effect. I also include a specification with a cohort course-set fixed effect to limit the course-set comparisons to within a cohort. The standard errors are clustered at the school level. Similar to the previous sorting equations, I present these coefficients graphically in Figure I.4. The corresponding regression tables are displayed in Appendix Tables A9-A12.

I run the covariate sorting equations from the previous section as well to examine which covariates predict having more same-race teachers and how those covariates change with the course-set fixed effect. I run the regressions below, limiting each regression to only one race of students to examine which covariates of students predict having more same-race teachers. The standard errors are clustered at the school level.

$$NumBlackTeach_i = \alpha_3 X_i + \kappa_s + \epsilon_{is} \quad (I.14)$$

$$\text{if } StuBlack_i = 1$$

$$NumHispTeach_i = \beta_3 X_i + \kappa_s + \epsilon_{is} \quad (I.15)$$

$$\text{if } StuHisp_i = 1$$

$$NumAsianTeach_i = \gamma_3 X_i + \kappa_s + \epsilon_{is} \quad (I.16)$$

$$\text{if } StuAsian_i = 1$$

$$NumWhiteTeach_i = \delta_3 X_i + \kappa_s + \epsilon_{is} \quad (I.17)$$

$$\text{if } StuWhite_i = 1$$

I present coefficients α_3 , β_3 , γ_3 , and δ_3 in Figure I.5. The corresponding regression tables are displayed in Appendix Tables A13-A16. In an ideal randomized experiment, these covariates

would be balanced. For example, a gifted Black student would ideally be no more or no less likely to have more Black teachers than a non-gifted Black student. These regressions allow one to examine how the balance of covariates changes with different fixed effect strategies.

Overall, in both Figures I.4 and I.5 that there is a general convergence of the coefficients that indicate non-random sorting toward zero, suggesting that there is a reduction in the non-random sorting of students to same-race teachers. In particular, nearly all of the covariates that predict having a same-race teacher converge to zero and are no longer statistically significant, bringing this natural experiment closer to a plausibly random distribution. The figures suggest course-set fixed effects strongly reduce the amount of endogenous sorting that occurs over no fixed effects or high school fixed effects.

6.2 Estimation

I use the course-set fixed effects to estimate the effect of matching with a same race teacher in high school in a linear dosage model. I estimate the following regression:

$$Y_i = \beta_1 BlackMatch_i + \beta_2 HispMatch_i + \beta_3 AsianMatch_i + \beta_4 WhiteMatch_i + \gamma_1 X_i + \kappa_s + \epsilon_{is} \quad (I.18)$$

In this regression, Y_i is an indicator for if student i graduated from high school within four years of 9th grade or enrolled within five years of 9th grade. X_i is a vector of student level characteristics, and κ_s is the course-set fixed effect, grouping students in the same high school who took identical courses. Each of the match coefficients are identified from students of the same-race having different teacher compositions conditional on their selected courses. The reference group for these match terms is in relation to a non-match, allowing for an easy to interpret coefficient. The match variables are in counts for the linear dosage model, so β_1 , β_2 , β_3 , and β_4 represent one additional race match in the 9th grade for Black, Hispanic, Asian, and White students, respectively. I do not include a high school fixed effect as it is implicitly nested within the course-set fixed effect. I include an additional specification with cohort-course-set fixed effects, limiting the comparison to students with identical course sets in the same 9th grade cohort.

The dosage model allows one to test marginal impact of same-race teachers. Whether or not there are increasing, constant, or diminishing marginal returns is of first order importance as it allows for researchers to develop concrete policy recommendations. For example, if there are increasing marginal returns to having same-race teachers, it could suggest evidence in favor of having as many same-race teachers as possible.

Another benefit to determining the marginal effects is that it gives testable implications for the mechanisms behind positive race match effects. Gershenson et al. (2018) develop a dosage model with testable implications for how race matching can impact students. One theory of race matching suggest that same-race teachers are more effective at communicating with students and expanding their worldview through culturally relevant pedagogy and cultural synchronization (Irvine (1990); Ladson-Billings (1995)). Another theory of race matching suggests that there is a role-model effect where teachers serve as role models to students, which could impact students by updating their inaccurate beliefs about returns to human capital. Minority students may have an inaccurate belief about being able to attend college despite having sufficient ability to do so and having a same-race teacher could update that belief.

The model developed by Gershenson et al. (2018) gives implications on how the marginal same-race teacher will impact educational outcomes with constant marginal returns to same-race teachers suggesting an increased effectiveness mechanism and diminishing marginal returns suggesting a role model effect. The intuition behind this implication is that increased effectiveness would be present no matter how many previous same-race teachers one had. On the other hand, role model effects work through changing a student's beliefs about themselves or the returns to education and that the 6th same-race teacher that a student had would presumably update their belief less than the 1st same-race teacher.

To test the non-linear effects of race-matching, I estimate a flexible dosage model, allowing for varying marginal effects. I create indicators for the number of same race teachers that a student could have. For Black, Hispanic, and White students the number of same-race teachers varies from zero to six, and for Asian students, it varies from zero to three. The reduction in potential matches for Asian students is lower simply because there are so few Asian students and teachers

that there are no Asian students in the sample with more than three Asian teachers. Specifically, I estimate the below equation:

$$\begin{aligned}
Y_i = & \sum_{k=1}^{k=6} \theta_k \mathbb{1}(BlackMatch_i=k) + \sum_{k=1}^{k=6} \lambda_k \mathbb{1}(HispanicMatch_i=k) \\
& + \sum_{k=1}^{k=3} \nu_k \mathbb{1}(AsianMatch_i=k) + \sum_{k=1}^{k=6} \eta_k \mathbb{1}(WhiteMatch_i=k) \\
& + \beta_1 X_i + \kappa_s + \epsilon_{is}
\end{aligned} \tag{I.19}$$

The notation is the same as the previous regressions. However, the interpretation on coefficients on the match terms changed. The omitted category for these match terms is students with no race matches, so the interpretation on θ_2 is the effect of 2 race matches for a Black student relative to a Black student with 0 race matches.

7 Long-term Results

7.1 Individual Results

I present the linear results in Table I.5 which shows the specifications with the course-set fixed effects and the cohort course-set fixed effects. The results for the course-set fixed effects and cohort course-set fixed effects are nearly identical. I present the coefficient for each race-match next to the race-specific mean for context to the size of the effect.

Black students become significantly more likely to enroll in college, driven by four-year college enrollment, after race matching in the 9th grade. One additional Black teacher for a Black student increases the likelihood of enrolling in a four-year college by 1 percentage point. Hispanic students benefit across all outcomes when race matching. Hispanic students matched with one additional Hispanic teacher increase their likelihood to graduate from high school by 0.7 p.p., likelihood to enroll in any college by 1.5 p.p., likelihood to enroll in a two-year college by 0.7 p.p., and likelihood to enroll in a four-year college by 0.9 p.p.

However, underrepresented minority students are not the only group to benefit from race matching in this linear model. Asian students become 1.8 p.p. more likely to graduate from high

school following an additional race match in 9th grade. Although, the confidence interval is so much wider on the estimate for an Asian match than the other races. Finally, White students become 0.7 p.p. more likely to enroll in college, and 0.4 p.p. to enroll in a two-year college after one additional race match in 9th grade. However, I will show in the Robustness Check section that the results for White students in the linear dosage model are not robust to Oster (2019) and her bounding methodology. The results for White students are the only long-term results that are not robust, suggesting that White students may not benefit from race-matching.

One potential downside to this linear model is that it imposes an assumption of constant marginal effects for the effect of race matching. This is particularly troublesome as the racial distribution of teachers is so skewed toward White teachers. This skewed distribution is evident in the summary statistics with White students having 3.7 race matches on average, compared to the 0.9 race matches Black students have on average. Given how skewed these distributions are, it is plausible that the effect varies accordingly with the amount of matches a student receives, which could also vary by race.

The functional form of the effect over a different amount of race matches is also of first order importance for determining policy implications. Another benefit to determining the functional form is that it would allow one to test different mechanisms of how race-matching impacts educational outcomes using the model developed by (Gershenson et al., 2018). In their model, constant marginal returns suggest that students improve with same-race teachers because teachers are more effective at teaching same-race students, while diminishing marginal returns would suggest that role model effects are present.

To test how marginal effectiveness of same-race teachers varies, I do not impose any functional form on the effect of additional race-matches, allowing the marginal effect of race-matching to vary. I run regression I.19 specified in the previous section creating indicators for each amount of race match for each race. I present the coefficients for the effect of race matching on high school graduation and college enrollment in Table I.6, and I plot the coefficients in Figure I.6. I present the coefficients for the effect of race matching on two- vs four-year enrollment Table I.7 and in

Figure I.7. To show the context of the distribution of race matches by race, I plot a histogram overlaid on Figure I.6 and Figure I.7 to show the support for each coefficient estimated.

In Figure I.6 and Figure I.7, the confidence intervals can be fairly large since the sample used to estimate each coefficient is smaller than in the linear or quadratic models, and many of the estimates are statistically indistinguishable from each other. However, looking at the point estimates can still be instructive in terms of the estimated effect. For all of the enrollment outcomes, there does appear to be diminishing marginal returns when examining the point estimates. For Black students enrolling in any college or a four-year college, there is a linear increase in the effectiveness until about the third race match when there is a plateauing of an effect. For Hispanic students enrolling in any college or a two-year college, there is a diminishing increase with a small negative marginal effect from the sixth match. Four-year enrollment for Hispanic students looks to be monotonically increasing with Hispanic matches but still with diminishing marginal effects. The effect on high school graduation for Black and Hispanic students is seemingly different from the enrollment outcomes. For both of Black and Hispanic students, the effect of race matches on high school graduation appears to small or zero until getting five or six race matches, at which point the point estimates shoot up.

For Asian students, it can be difficult to assess the shape of the curve as the sample size gets increasing small when breaking the groups up further. Finally, for White students, there are clear diminishing marginal returns with no coefficient on the match terms being significantly different from zero for all outcomes. The non-linear results for White students seem to contradict the linear results where there were significant effects for White students in any college enrollment and two-year college enrollment. One potential way of reconciling this is by examining the distribution of matches for White students. About 75% of race matches for White students are between one and four where there is a clear positive relationship for these outcomes and race matches. However, for the fifth and 6th matches, the marginal effect becomes negative. This non-linear relationship between race matches and marginal effects underlies the importance of allowing the model to be flexible without imposing linearity.

In particular, the diminishing marginal returns in race matches and Gershenson et al. (2018)

suggest that role model effects are present and are potentially acting as a mechanism for same-race teachers increasing student achievement. These results give a policy recommendation of increased hiring of Black and Hispanic teachers to help Black and Hispanic students while minimally impacting White students.

7.2 Heterogeneity

Next, I explore if race matching in a subject in high school makes a student more likely to major in the subject in college. The distribution of college degrees is not equitable with Black and Hispanic students systematically less likely to major in STEM than their White and Asian counterparts. STEM degrees earn a larger wage premium, so this inequality in STEM degrees further exacerbates wage disparities (Altonji, Blom and Meghir, 2012). I also examine two other majors Social Sciences and English/Writing. I define a STEM degree as a degree with a six-digit Classification of Instructional Programs (CIP) code matching the list of STEM majors designated by the Department of Homeland Security¹³. For English/Writing and Social Sciences, I use two-digit CIP codes to define the major choice¹⁴.

I examine how race matching within certain subjects affects a student's likelihood of majoring in STEM in college as a freshman, conditional on their 9th grade course-set. One may expect there to be an effect for Black and Hispanic students for race matching in STEM courses because of the role model effect, which may be more salient for certain demographics in areas that they are underrepresented like Black and Hispanic students in STEM. I present the results from matching in a given subject in Table I.8.

Black and Hispanic students increase their likelihood of majoring STEM by 0.7 and 0.6 p.p., respectively, with one additional 9th grade same-race STEM teacher. One additional White English teacher increases the likelihood of a White student majoring in English/Writing by 0.3 p.p.. Finally, an additional same-race Social Science teacher for a Hispanic student increases their like-

¹³<https://www.ice.gov/sites/default/files/documents/Document/2016/stem-list.pdf>

¹⁴For English/Writing courses, I focus on CIP codes involving written or oral communication. Specifically, I use codes 09 and 23. For Social Sciences, I use codes 42, 45, and 59.

likelihood of majoring in Social Sciences by 0.3 p.p.. These heterogeneity results suggest an increased need to hire Black and Hispanic STEM teachers in high school.

Another potential source of heterogeneity is student ability. I examined heterogeneity using 8th grade reading test quartiles at the course-level to tease out the differences in effects for students with different underlying ability. I conduct a similar analysis, splitting the sample into four quartiles of 8th grade reading test scores. I present the analysis in Table I.9.

In Panel A, I show the heterogeneity analysis for high school graduation and college enrollment in any college, and in Panel B, I present the results for college enrollment separately for two- and four-year colleges. For Black students, there is a significant increase in the likelihood to graduate high school with an additional race match when they are in the top half of the 8th grade reading test score distribution. For college enrollment, the effect is spread out evenly across the distribution with the top three quartiles becoming more likely to enroll in a four-year college and the bottom quartile becoming more likely to enroll in a two-year college.

For Hispanic students, the same general pattern can be seen with the top section of the distribution becoming more likely to graduate high school and enroll in a four-year college from an additional race match and the bottom two quartiles becoming more likely to enroll in a two-year college. Finally, the race math effect is positively correlated with 8th grade reading test scores for college enrollment for Hispanic students. White students also have a positive correlation in race match effect with the test score distribution for college enrollment.

Asian students see a different pattern for their outcomes. The significant and positive effects come from the bottom of the test score distribution. Asian students in the bottom quartile of 8th grade reading scores become much more likely to graduate from high school or enroll in a two-year college with an additional Asian teacher, but none of the other students are significantly affected. For college enrollment, there is actually a negative effect for a race match for an Asian student at the top quartile of the distribution, driven by a decrease in two-year enrollment. This significant decrease in two-year enrollment is potentially concerning because it is not offset by an increase in four-year college enrollment.

Overall, this heterogeneity analysis sheds further light on the potential mechanism behind

role model effects. Given the results of the non-linear dosage model and Gershenson et al. (2018) model, role model effects are present due to the diminishing marginal returns of race matching. One potential way the role model effect could work is through updating students' beliefs about returns to education or their belief in their own abilities. Students may not have attended college despite having sufficient ability to do so because of an incorrect belief about their own ability. Given the race match effects for Black and Hispanic students is concentrated at the top half of the distribution, it gives further credence and evidence to a role model effect dominating.

7.3 Thought Experiment and Simulation

Finally, I take my estimates from the previous section and do a back of the envelope calculation based on the following thought experiment. Suppose that the racial distribution of teachers at each school matched the racial distribution of students. For example, if a school has 10% Black students, 50% Hispanic students, 35% White students, and 5% Asian students, then suppose that the campus also had 10% Black teachers, 50% Hispanic teachers, 35% White teachers, and 5% Asian teachers.

To achieve this, I randomly assign races to teachers with weights such that each school should achieve a representative teaching population. This change would be an extremely large and politically infeasible policy, and that many assumptions underlie this analysis. In particular, the estimates for race-matching would likely change, and students would likely endogenously move schools as a function of this policy as well. While the simulation may not be implementable in real life, it can still be instructive to think about how a representative teaching population could affect students and educational outcomes.

In Figure I.8, I show how the racial distribution for students at a the high school level compares to that of the racial distribution for teachers with a dashed line showing what a representative school would look like. If a high school is above the line, then their students are underrepresented by the teaching population. Black students and Hispanic students are underrepresented at 70% and 96% of high schools, respectively. However, White students are over represented by the teaching population at 97% of high schools. This thought experiment would in effect reduce the

number of White teachers at 97% of schools and increase them in the other 3% of schools. Black students will see more Black teachers at 70% of schools and fewer in the other 30% of schools, which could hurt some of the Black students at those schools.

I run 5,000 Monte Carlo simulations randomizing teacher races such that teachers are representative at the school level. On average, these simulations result in having 1,601 more Black teachers, 10,705 more Hispanic teachers, 673 more Asian teachers, and 12,979 fewer White teachers. I recover how many race matches each race had in the average simulation and multiply the change from the simulation by my point estimates shown in Table I.5. I present the change in the average number of race matches in Figure I.9, which shows that in the average simulation that Black, Hispanic, and Asian students have more race matches while White students have fewer. For example, in the observed data Hispanic students have 1.36 race matches on average, and in the simulation, they have 2.88 race matches on average. I take the difference between them and multiply by the point estimates in my results to recover a back of the envelope calculation. I calculate how many additional students would graduate high school and enroll in college for each race group and how the race-specific rates change. I present those results in Table I.10.

There are large gains for Hispanic students in this back of the envelope calculation. In Panel A, I present how many additional or fewer students achieve each educational outcome and in Panel B, I present how those changes would move each race-specific rate. For instance, I find that the average simulation would increase Hispanic college attendance by about 5,600 students shown in Panel A, and this change represents a 2.3 p.p. (5.6%) increase in the rate of college enrollment for Hispanic students. There are smaller changes for Black and Asian students for two reasons. The first is that there the effects of race matching are smaller for these groups and the change in the number of teachers is relatively small compared to Hispanic students. The effects for White students are also much smaller than Hispanic students, but they are negative. The average simulation would suggest that about 1,500 fewer White students would enroll in college, driven mostly by reduced attendance in two-year institutions. This change represents a 0.9 p.p. (1.7%) reduction in college attendance.

The average number of race matches changes, but the distribution of race matches also changes

dramatically, which I show in Figures I.6 and I.7 matters for understanding the effect of race matches. I present the simulations and back of the envelope calculations using the estimates from the non-linear results. I show how the distribution of race matches changes with the simulation in Figure I.10. Minority students have their distribution of matches shifted toward the right with the opposite being true for White students. I plot the effects for the non-linear effects with how the distribution of race matches changes in Figures I.11 and I.12. I multiply the change in the distribution by the coefficient for the effect of each race match to get at the aggregate change. For example, there are more Hispanic students receiving 4 race matches, so I multiply the increase in number of Hispanic students receiving four race matches by the effect of having four race-matches. I present the back of the envelope calculations in Table I.11. The results are largely the same as the back of the envelope calculations in Table I.10 from the linear results, but they are slightly smaller, consistent with diminishing marginal returns from race matching.

In aggregate, these results show large, positive changes in high school graduation and college enrollment driven mostly by Hispanic students. In total, I find that a representative teaching population would increase high school graduation for about 2,500 students and college enrollment for about 4,000 students. This change would represent a 0.5 p.p. increase in the high school graduation rate and a 0.8 p.p. increase in the college enrollment rate. Overall, these results suggest that a policy to make the teaching population more representative would be a net-positive intervention, improving minority student outcomes more than it would hurt White student outcomes.

8 Robustness Checks

8.1 Oster (2019) Bounding

My estimation and identification strategy use institutional knowledge paired with fixed effects to minimize endogenous sorting of students and teachers. However, I am unable to entirely eliminate non-random sorting. There is a non-trivial concern that I am failing to account for some unobservable characteristics that students are sorting along that could result in an omitted variable bias that is biasing my estimate of the true effect of race matching in high school. One

potential way to address this concern is to implement the bounding methodology described in Oster (2019), which will allow me to determine how serious this selection could be.

Oster (2019) demonstrates that one is able to evaluate a finding's robustness to omitted variable bias by examining the coefficient and R-squared stability and proves that a consistent bias-adjusted treatment estimation is possible under two assumptions. The first assumption one makes is the relative selection of unobservable and observable characteristics, denoted by δ . Given one cannot examine unobservable selection, one must make an assumption about how much selection occurs on unobservable characteristics. The second assumption is the maximum value of R-squared. She argues for the first assumption that it is reasonable to assume that there is equal selection of observable and unobservable characteristics, i.e. $\delta = 1$. For the maximum value of R-squared, she suggests $R_{max}^2 = \min\{\Pi R^2, 1\}$ is reasonable, with Π being a scalar. She shows that randomized results indicate that $\Pi = 1.3$ is appropriate (Oster, 2019). These two assumptions give a bounding set that for a given treatment effect defined as $\Delta = [\tilde{\beta}, \beta^*(R_{max}^2, \delta = 1)]$, such that $\tilde{\beta}$ gives the effect with full controls and β^* gives the bias-adjusted treatment effect.

Ultimately, my fixed effect strategy is leveraging variation at the course level and using fixed effects is akin to selection on observables, making the use of Oster Bounds appropriate. To evaluate how robust the findings are when selecting on student covariates and the high school by year by course or course set fixed effects, I calculate the bounding set Δ for each significant finding. Oster (2019) gives two potential ways of evaluating robustness: 1. if 0 is excluded from bounding set Δ , and 2. if β^* is within the 99.5% confidence interval of $\tilde{\beta}$. I present the bounding sets for each significant result and whether they are robust to either measurement in Table I.12 and Table I.13.

Out of the 13 significant results examined in the Oster (2019) Bounding analysis, 12 of the results are robust to the bounding set excluding zero, suggesting that the vast majority of the significant results are still different from zero when adjusting for selection on observable and unobservable characteristics. Out of the 13 results, 9 are also robust to being within the 99.5% confidence interval of the original estimate, suggesting that they are sufficiently close to the original estimate. I present the bounding estimates graphically showing the original estimate

and the bias-adjusted estimate and how they compare to the two robustness criteria. I present the short-term course-level results on test scores and pass rates in Figure I.13 and the long-term results on high school graduation and college enrollment in Figure I.14.

It is important to account for the potential omitted variable bias of some unobservable variable driving my results. Given that I cannot fully eliminate sorting along race or other observable characteristics using my fixed effects strategy, it is likely there is also selection on unobservable characteristics. The bounding sets allow me to quantify how important the selection of characteristics is given certain assumptions like equal selection of observable and unobservable characteristics. The table and figures show that my results are largely the same when trying to account for unobservable selection using Oster (2019) with some exception. In particular, the results for test scores and the long-term results for White students are not robust. This lack of robustness for the significant linear effects for White students gives further credence to the non-linear results for White students suggesting there is no significant benefit to White students matching with White teachers.

8.2 Expanding Course-Set to 10th grade

In the main analysis, I use the 9th grade courses that students take to create a course-set to compare students that took identical courses. I focus on 9th grade courses because race matches in 10th, 11th, and 12th grade could be endogenous to the matches in 9th grade if they are a function of the race matches in 9th grade. However, it is possible to still examine the matches in later matches as well. I create 10th grade course-sets to examine if the effect of race matching could vary with a student's age or grade level. I can extend this analysis to 11th and 12th grade as well, but as the grade level becomes higher, the sample size becomes smaller as some students drop out and course offerings become larger, leading to more students without another student with an identical course-set and therefore lacking a comparison.

In Table I.14, I present the results using the 10th grade course-set fixed effects in the first panel and the results using a 9th and 10th grade course-set fixed effect in the second panel. The first set of results compares students with identical 10th grade course selections but have different race

matches. The second set of results compares students with identical 9th and 10th grade course selections but have different race matches.

The results from using the 10th grade course sets fixed effects are extremely similar to the results using the 9th grade course sets. Black and Hispanic students have significantly better long-term outcomes from race-matches in the 10th grade. The magnitude of the results is slightly higher than the effect of 9th grade race-matches, but they are statistically indistinguishable from each other. Asian students appear to not benefit from 10th grade race-matches, while significantly becoming more likely to graduate high school from 9th grade race-matches. Finally, White students become more likely to enroll in college following 10th grade race matches in a slightly smaller magnitude compared to 9th grade race matches.

9 Discussion

One noticeable difference between the short-term course-level outcomes and the longer-term individual-level outcomes is the difference in who is affected. Black students benefit from matching in both long- and short-term outcomes, but this is not true for all of the students. Asian students benefit in the short-term from a race match but are not as affected in the long-term, which is the reverse for Hispanic students receiving a race match, who benefit in the long-term from a race match but do not benefit in the short-term.

There exists a disconnect between the short-term course results and long-term individual results. Black and Asian students benefit modestly in the short-term by having slightly higher test scores and pass rates. However, Black students see much larger gains in the long-term from race matches with large increases in their likelihood to enroll in college that seem disproportionately large to their short-term benefits. Black students become 0.9 p.p. more likely to pass their classes with Black teachers and 1 percentage point more likely to enroll in a four-year college after one additional 9th grade Black teacher. These outcomes are different educational achievement measures and are not directly comparable, but it still seems unlikely that improving the pass rate by a small amount would result in a large effect size for college enrollment. It could mean that

same-race teachers also increase non-cognitive measures of students that impact expectations and beliefs that are not directly picked up in measurement like test scores or pass rates. This implication is consistent with past research into expectations, behavior, and beliefs¹⁵.

This disconnect between long- and short-term effects seems counterintuitive as one may expect a student to benefit the most the closer they are to the treatment than further away. However, this finding is consistent with the only other paper in the literature examining long term effects of race matching. Gershenson et al. (2018) find a 4 percentage point increase in college enrollment with an additional Black K-3 teacher for Black students, while I find a 1 percentage point increase from an additional 9th grade Black teacher. My results taken with Gershenson et al. (2018) suggest that the effect of race matching may be stronger in the long-term. This difference between short-term and long-term effects suggest different mechanisms affecting short- and long-term outcomes.

One potential reason for the differences in results is that in the short-term outcomes the relative frequency of teachers is important for having an effect. Black and Asian students are the smallest minority groups with Hispanic and White students comprising the two largest ethnic groups in Texas. There are also many more Hispanic and White teachers than there are Asian and Black teachers. However, there are no significant effects for Asian students in the long-term outcomes for college enrollment. The long-term linear effects of race matching for college enrollment had effects for each race except that with the highest achievement - Asian students.

It may be that in the short-term there are strong effects from “culturally relevant pedagogies” such as having different interactions that are culturally relevant to Black and Asian students that only same-race teachers would understand. There has been research into how culturally relevant pedagogies affect Black students (Irvine (1990); Ladson-Billings (1995)). Another potential effect from race matching is a role model effect wherein role models may get students to update their beliefs about human capital accumulation (Marx and Roman (2002); Dee (2005)). Research shows that having a same-race teacher can raise a student’s expectations on achieving higher education (Fox (2015); Papageorge, Gershenson and Kang (2018)). Importantly, these theories about how

¹⁵(Fox (2015); Papageorge, Gershenson and Kang (2018))

race matching could improve students outcomes are not mutually exclusive but likely work in conjunction with one another, with a culturally relevant pedagogy feeding into role model effects and vice-versa.

Overall, the results are consistent with a story of culturally relevant pedagogies more strongly affecting short-term outcomes and role model effects impacting longer-term outcomes. The culturally relevant pedagogies would likely be strongest with students that have the least representation for teachers as is the case for Black and Asian students, while the role model effects would impact the lower achieving groups more such as Black, Hispanic, and White students. Specifically, the impact of race matching for long-term outcomes like college enrollment are largest for Black and Hispanic students who would have the biggest impact on their educational expectations. My results further validate this hypothesis when looking at the non-linear effects of dosage for race matching in long-term outcomes. Following the model developed in Gershenson et al. (2018), diminishing marginal returns of same-race teachers would imply that role-model effects dominate over increased effectiveness of same-race instruction.

Racial gaps in educational attainment have been documented in many different settings, and Texas is no exception. Black and Hispanic students lag behind their White peers who also lag behind their Asian peers in educational outcomes. Race-matching appears to significantly decrease the race gap between underrepresented minorities and White students. For instance, an additional Black teacher match reduces the Black-White college enrollment gap by 11%, and an additional Hispanic teacher match reduces the Hispanic-White high school graduation and college enrollment gap by 14% and 11%, respectively.

Race-matching for White students on the other hand appears to have no robust effect on White student outcomes. While there is a positive effect for White students in the linear dosage model, I show in the Robustness Check section that those results are not robust to Oster (2019) and her bounding methodology. Specifically, the long-term outcomes for White students are the only results that are not robust to the bounding strategy. When relaxing the linearity assumption and allowing the effect to be more flexible, White students no longer benefit from race-matching. Overall, the evidence in this paper suggests that White students do not benefit significantly from

race-matching and that one could replace White teachers with more Black and Hispanic teachers to the benefit of Black and Hispanic students without making White students any worse off.

The effect sizes of the race matches for Black and Hispanic students seem fairly large. The results indicate that one additional Black match increases college enrollment for Black students by 1 percentage point, and one additional Hispanic match increases college enrollment for Hispanic students by 1.5 p.p.. It's difficult to compare these results to the literature as there is only one other paper that directly observes long-term outcomes from matches. Gershenson et al. (2018) uses the Tennessee STAR randomized teacher-student assignment in elementary school to examine race matches of Black students and teachers. The randomization element should create unbiased estimates of these long-term effects of matches, providing an excellent comparison. They find that Black students randomly assigned to one Black teacher in grades K-3 are 4 p.p. more likely to enroll in college, an effect size about four times my effect size. It's not an exact comparison though as Gershenson et al. (2018) look at elementary school matching, and I examine high school race matching. This distinct difference in timing of the race-matches informs the broader education literature as well. Heckman argues that treatment effects can have larger impacts when children are younger as these skills can build on top of each other (Heckman 1999). One may be concerned that in high school it is too late to change the trajectory of a student's educational career from additional same-race teachers. I confirm Heckman's hypothesis in this setting showing the effect sizes are smaller but are still significant in high school. Using Gershenson et al. (2018) and their randomization as a reference point for an earlier intervention suggest that I find a plausible effect size.

10 Conclusion

I present evidence showing that race matching in high school can significantly impact racial gaps that exist in high school and college achievement. I control for non-random sorting better than past studies by exploiting institutional details and using course-level fixed effects. I reduce non-random sorting of students to same-race teachers by about 99% for Black, Hispanic, and

White students. Only Gershenson et al. (2018) has examined the effects of race matching on high school graduation and college enrollment but does not examine Hispanic or Asian students, which my paper sheds new light on. I present the first evidence of high school student-teacher race matching effects. Race matching for Black and Hispanic students significantly decreases race gaps. An additional same-race 9th grade teacher for Black and Hispanic students increases their likelihood to enroll in college by 1 and 1.5 p.p., respectively. In my heterogeneity analysis, I also show that Black and Hispanic students become significantly more likely to major in a STEM field as freshmen in college after having a Black or Hispanic STEM teacher.

While I find significant effects for White students in the linear dosage model, these results are not robust to the bounding methodology outlined in Oster (2019). Furthermore, the non-linear dosage model shows that White students do not benefit from race-matching across all outcomes. These results taken with the results for Black and Hispanic students suggest that hiring more Black and Hispanic teachers could greatly improve academic achievement for Black and Hispanic students while having minimal negative tradeoffs for White students. A policy aimed at decreasing racial gaps in education should target training and hiring Black and Hispanic teachers with a focus on STEM teachers.

Tables

Table I.1. Descriptive Statistics

Panel A: Student Characteristics				
Variable	Black Mean	Hispanic Mean	Asian Mean	White Mean
HS Grad	0.777	0.787	0.898	0.830
Enroll Any	0.455	0.409	0.662	0.532
Enroll two-year	0.277	0.280	0.353	0.318
Enroll four-year	0.188	0.142	0.423	0.234
STEM Major	0.073	0.083	0.260	0.114
Test Z-Score	-0.107	-0.049	0.834	0.401
Pass Rate	0.872	0.858	0.970	0.939
Female	0.499	0.494	0.489	0.488
At Risk	0.696	0.702	0.301	0.425
Gifted	0.059	0.085	0.250	0.114
Free/Reduced Price Lunch	0.738	0.800	0.368	0.425
n	88,668	323,067	26,438	215,661
Panel B: Student and Teacher Composition				
Variable	Black Mean	Hispanic Mean	Asian Mean	White Mean
Student Composition	0.137	0.498	0.041	0.332
Teacher Composition	0.091	0.183	0.023	0.702

Note: Descriptive statistics showing student achievement, characteristics, and composition for Texas high school students in 2012 and 2013 9th grade cohort. Panel A shows student characteristics and outcomes at the student level. Panel B shows student and teacher racial composition at the state level. Data comes from the Texas Education Research Center linking Texas public high school data to Texas and national college data.

Table I.2. Race-Matching Effects on Course-level Outcomes

VARIABLES	(1) Test Z-Score		(2) Pass	
	Estimate	Mean	Estimate	Mean
Black Match	0.013*** (0.004)	-0.107	0.009*** (0.001)	0.872
Hispanic Match	0.003 (0.004)	-0.049	0.001 (0.001)	0.858
Asian Match	0.070*** (0.021)	0.834	0.014*** (0.002)	0.970
White Match	0.001 (0.004)	0.401	-0.0003 (0.0007)	0.939
Observations	2,268,544		8,955,014	
R-squared	0.786		0.443	

Note: This table shows the effect of race matching at the course level on course-level outcomes using student, teacher, and high school by year by course fixed effects. There are fewer observations for the test scores as standardized tests are only administered in five courses while every course designates whether a student passes. “Test Z-Score” measure is in terms of standard deviations, and the Match terms are interpreted as changes in a standard deviation. Pass is an indicator for if a student passes the course, and the Match terms are interpreted as a percentage point change. Race specific means are next to the estimated effect for context. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.3. Heterogeneity Effects of Race-Matching by 8th Grade Reading Test Scores

VARIABLES	(1) Test Z-Score				(2) Pass			
	Black Match	Hispanic Match	Asian Match	White Match	Black Match	Hispanic Match	Asian Match	White Match
8th Grade Reading Test Scores								
Top Quartile	0.003 (0.009)	-0.004 (0.007)	0.052** (0.022)	0.006 (0.006)	0.005** (0.002)	-0.001 (0.001)	0.015*** (0.003)	-0.001 (0.001)
2nd Quartile	0.012* (0.007)	-0.001 (0.005)	0.054* (0.032)	-0.004 (0.006)	0.008*** (0.002)	-0.0003 (0.001)	0.011*** (0.003)	0.0001 (0.001)
3rd Quartile	0.014** (0.006)	0.004 (0.005)	0.098*** (0.035)	0.003 (0.006)	0.009*** (0.002)	0.0016 (0.001)	0.015*** (0.004)	-0.001 (0.001)
Bottom Quartile	0.016** (0.007)	0.010* (0.006)	0.095* (0.057)	-0.0004 (0.006)	0.013*** (0.002)	0.003** (0.001)	0.017*** (0.003)	0.001 (0.002)
Observations	2,268,544				8,955,014			
R-squared	0.786				0.443			

Note: This table shows the effect of race matching at the course level on course-level outcomes using student, teacher, and high school by year by course fixed effects. The regressions are broken into quartiles of 8th grade math test scores. There are fewer observations for the test scores as standardized tests are only administered in five courses while every course designates whether a student passes. “Test Z-Score” measure is in terms of standard deviations, and the Match terms are interpreted as changes in a standard deviation. Pass is an indicator for if a student passes the course, and the Match terms are interpreted as a percentage point change. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.4. Descriptive Statistics: Dosage Model

Panel A: Student Characteristics				
Variable Mean	Black	Hispanic	Asian	White
HS Grad	0.597	0.668	0.718	0.711
Enroll Any	0.488	0.438	0.678	0.562
Two-Year Enroll	0.291	0.298	0.357	0.33
Four-Year Enroll	0.21	0.157	0.443	0.255
STEM Major	0.081	0.091	0.27	0.122
Total Teachers	4.2	4.3	4.2	4.3
Number of Race Matches	0.902	1.37	0.155	3.74
Obs	66,826	246,450	22,302	175,912
Panel B: Student and Teacher Composition				
Variable Mean	Black	Hispanic	Asian	White
Student Composition	0.125	0.460	0.042	0.329
Teacher Composition	0.089	0.182	0.021	0.703

Note: Descriptive statistics showing student achievement, characteristics, and composition for Texas high school students in 2012 and 2013 9th grade cohort in the dosage model. Panel A shows student characteristics and outcomes at the student level. Panel B shows student and teacher racial composition at the state level. Data comes from the Texas Education Research Center linking Texas public high school data to Texas and national college data.

Table I.5. Linear Dosage Effects of Race Matching of 9th Grade Teachers

Panel A: Course-Set Fixed Effects								
VARIABLES	(1) HS Grad		(2) Enroll Any		(3) Enroll Two-Year		(4) Enroll Four-Year	
	Estimate	Mean	Estimate	Mean	Estimate	Mean	Estimate	Mean
Black Match	0.003 (0.003)	0.597	0.010*** (0.003)	0.488	0.003 (0.002)	0.291	0.010*** (0.002)	0.210
Hispanic Match	0.007*** (0.002)	0.668	0.015*** (0.003)	0.438	0.007** (0.003)	0.298	0.009*** (0.002)	0.157
Asian Match	0.018** (0.008)	0.718	-0.015 (0.015)	0.678	-0.002 (0.012)	0.357	-0.011 (0.010)	0.443
White Match	0.001 (0.002)	0.711	0.007*** (0.002)	0.562	0.004** (0.002)	0.330	0.002 (0.001)	0.255
Observations	514,501		514,501		514,501		514,501	
R-squared	0.36		0.232		0.122		0.257	
Panel B: Cohort Course-Set Fixed Effects								
VARIABLES	(1) HS Grad		(2) Enroll Any		(3) Enroll Two-Year		(4) Enroll Four-Year	
	Estimate	Mean	Estimate	Mean	Estimate	Mean	Estimate	Mean
Black Match	0.003 (0.003)	0.597	0.010*** (0.003)	0.488	0.003 (0.003)	0.291	0.009*** (0.002)	0.210
Hispanic Match	0.007*** (0.002)	0.668	0.016*** (0.003)	0.438	0.008*** (0.003)	0.298	0.010*** (0.002)	0.157
Asian Match	0.017** (0.008)	0.718	-0.012 (0.015)	0.678	-0.003 (0.011)	0.357	-0.01 (0.010)	0.443
White Match	0.002 (0.002)	0.711	0.007*** (0.002)	0.562	0.003* (0.002)	0.330	0.002 (0.002)	0.255
Observations	506,034		506,034		506,034		506,034	
R-squared	0.376		0.248		0.141		0.274	

Note: This table provides the results from the linear dosage model for 9th grade race matches conditional on course-set or cohort course-set fixed effects. The fixed effects make comparisons between students that took identical courses in the same high school but have different race teachers. The interpretation on the Match coefficient is the effect of one additional same-race teacher. Race specific means are next to the estimate for context. Panel A displays the effect using course-set fixed effects while Panel B shows the cohort course-set fixed effects. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.6. Non-linear Match Effects for HS Graduation and College Enrollment

VARIABLES	(1) HS Grad				(2) Enroll Any			
	Black Match	Hispanic Match	Asian Match	White Match	Black Match	Hispanic Match	Asian Match	White Match
Match == 1	0.003 (0.005)	0.006* (0.003)	0.019** (0.009)	-0.02 (0.015)	0.007 (0.006)	0.014*** (0.004)	-0.003 (0.012)	-0.027 (0.021)
Match == 2	-0.001 (0.007)	0.011** (0.006)	0.023 (0.028)	0.008 (0.015)	0.024*** (0.008)	0.034*** (0.008)	-0.076 (0.060)	-0.003 (0.020)
Match == 3	0.004 (0.008)	0.014* (0.009)	0.075 (0.076)	0.015 (0.015)	0.036*** (0.010)	0.046*** (0.011)	-0.190* (0.110)	0.011 (0.019)
Match == 4	0.009 (0.014)	0.016 (0.010)	- -	0.017 (0.015)	0.025* (0.014)	0.056*** (0.015)	- -	0.022 (0.019)
Match == 5	0.057** (0.029)	0.023* (0.013)	- -	0.005 (0.015)	0.047* (0.028)	0.062*** (0.019)	- -	0.015 (0.020)
Match == 6	0.087** (0.035)	0.045** (0.018)	- -	-0.005 (0.018)	0.012 (0.047)	0.056** (0.024)	- -	-0.002 (0.023)
Observations	514,501				514,501			
R-squared	0.360				0.232			

Note: This table shows the results of the non-linear dosage model for 9th grade race matches conditional on course-set on high school graduation and any college enrollment. I regress indicators for each number of race match on outcomes allowing the functional form to be flexible to test for non-linearity. The omitted group for each race is the no matches group, putting the coefficients in reference to no matches. No Asian students have more than 3 race matches. The fixed effects make comparisons between students that took identical courses in the same high school but have different race teachers. The coefficients are plotted in Figure I.6. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.7. Non-linear Match Effects for Two- and Four-Year College Enrollment

VARIABLES	(3) Enroll Two-Year				(4) Enroll Four-Year			
	Black Match	Hispanic Match	Asian Match	White Match	Black Match	Hispanic Match	Asian Match	White Match
Match == 1	-0.002 (0.005)	0.006* (0.004)	-0.003 (0.011)	-0.012 (0.019)	0.017*** (0.005)	0.008*** (0.003)	0.008 (0.010)	-0.025 (0.016)
Match == 2	0.002 (0.007)	0.020*** (0.006)	-0.003 (0.049)	0.003 (0.018)	0.035*** (0.006)	0.017*** (0.006)	-0.121*** (0.022)	-0.014 (0.014)
Match == 3	0.012 (0.011)	0.021** (0.009)	-0.035 (0.064)	0.011 (0.018)	0.027*** (0.008)	0.028*** (0.007)	-0.08 (0.097)	-0.003 (0.014)
Match == 4	0.013 (0.013)	0.022* (0.011)	- -	0.017 (0.018)	0.013 (0.010)	0.038*** (0.010)	- -	-0.002 (0.014)
Match == 5	0.039** (0.019)	0.02 (0.016)	- -	0.013 (0.018)	0.022 (0.018)	0.047*** (0.014)	- -	-0.007 (0.015)
Match == 6	0.001 (0.054)	0.007 (0.019)	- -	0.001 (0.021)	0.004 (0.018)	0.052*** (0.018)	- -	-0.016 (0.017)
Observations	514,501				514,501			
R-squared	0.122				0.258			

Note: This table shows the non-linear dosage model for 9th grade race matches conditional on course-set on two- versus four-year college enrollment. I regress indicators for each number of race match on outcomes allowing the functional form to be flexible to test for non-linearity. The omitted group for each race is the no matches group, putting the coefficients in reference to no matches. Asian students do not have more than 3 race matches. The fixed effects make comparisons between students that took identical courses in the same high school but have different race teachers. The coefficients are plotted in Figure I.7. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.8. Dosage Effects of Race Matching in STEM Courses

VARIABLES	(1) STEM Major	(2) Writing Major	(3) Social Science Major
Black Subject Match	0.007*** (0.003)	0.001 (0.001)	0.001 (0.002)
Hispanic Subject Match	0.006*** (0.002)	-0.001* (0.001)	0.003** (0.001)
Asian Subject Match	-0.006 (0.010)	0.005 (0.007)	-0.007 (0.008)
White Subject Match	0.0004 (0.002)	0.003*** (0.001)	0.00001 (0.001)
Observations	514,501	514,501	514,501
R-squared	0.174	0.091	0.096

Note: This table show the results from the dosage model for 9th grade race matches examining heterogeneity in subject conditional on course-set fixed effects. The fixed effects make comparisons between students that took identical courses in the same high school but have different race teachers. The interpretation on the Match coefficients is the effect of one additional same-race teacher in a given subject such as STEM, English, or Social Science. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.9. Heterogeneity of 8th Reading Scores in Dosage Model

Panel A: HS Grad and Enrollment								
VARIABLES	(1)				(2)			
	Effect of Race Match on HS Grad				Effect of Race Match on Enroll Any			
	Black	Hispanic	Asian	White	Black	Hispanic	Asian	White
8th Grade Reading Test Scores								
Top Quartile	0.008** (0.004)	0.009*** (0.002)	0.002 (0.012)	0.003* (0.002)	0.012** (0.005)	0.023*** (0.004)	-0.054*** (0.016)	0.010*** (0.002)
2nd Quartile	0.011*** (0.004)	0.008*** (0.002)	0.014 (0.012)	0.002 (0.002)	0.009*** (0.003)	0.016*** (0.004)	0.004 (0.020)	0.009*** (0.002)
3rd Quartile	-0.001 (0.003)	0.008*** (0.002)	0.015 (0.014)	-0.001 (0.002)	0.011*** (0.003)	0.014*** (0.003)	0.037* (0.020)	0.004** (0.002)
Bottom Quartile	0.0004 (0.004)	0.002 (0.003)	0.076*** (0.019)	-0.001 (0.002)	0.010*** (0.004)	0.010*** (0.004)	0.008 (0.022)	0.001 (0.002)
Observations	514,501				514,501			
R-squared	0.361				0.232			
Panel B: Two- vs Four- Year Enrollment								
VARIABLES	(3)				(4)			
	Effect of Race Match on Enroll Two-Year				Effect of Race Match on Enroll Four-Year			
	Black	Hispanic	Asian	White	Black	Hispanic	Asian	White
8th Grade Reading Test Scores								
Top Quartile	0.005 (0.004)	0.006** (0.003)	-0.036*** (0.014)	0.005*** (0.002)	0.012*** (0.004)	0.018*** (0.003)	-0.011 (0.016)	0.003* (0.002)
2nd Quartile	-0.006* (0.003)	0.005 (0.003)	-0.008 (0.016)	0.004* (0.002)	0.023*** (0.003)	0.013*** (0.002)	0.02 (0.016)	0.004*** (0.002)
3rd Quartile	0.0003 (0.003)	0.008*** (0.003)	0.047** (0.020)	0.003 (0.002)	0.011*** (0.002)	0.007*** (0.002)	-0.007 (0.022)	0.001 (0.002)
Bottom Quartile	0.011*** (0.003)	0.008*** (0.003)	0.053** (0.024)	0.003 (0.002)	-0.0004 (0.002)	0.003 (0.002)	-0.076*** (0.013)	-0.003 (0.002)
Observations	514,501				514,501			
R-squared	0.123				0.259			

Note: This table presents the linear dosage model for 9th grade race matches conditional on course-set effects examining heterogeneity in ability. The regressions are broken into quartiles of 8th grade math test scores. The fixed effect makes comparisons between students that took identical courses in the same high school but have different race teachers. The interpretation on the Match coefficient is the effect of one additional same-race teacher for a given student in a 8th grade math score quartile. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.10. Back of Envelope Calculations from Simulations

Panel A: Change in Number of Students				
	HS Grad	Enroll Any	Enroll Two	Enroll Four
Black	68.6	228.5	68.6	228.5
Hispanic	2629.1	5633.8	2629.1	3380.3
Asian	188.3	-156.9	-20.9	-115.1
White	-238.4	-1668.5	-953.4	-476.7
Total Minority Change	2886.0	5705.5	2676.8	3493.8
Total Change	2579.0	3808.4	1654.8	2788.5
Panel B: Change in Rate				
	HS Grad	Enroll Any	Enroll Two	Enroll Four
Black	0.001	0.003	0.001	0.003
Hispanic	0.011	0.023	0.011	0.014
Asian	0.008	-0.007	-0.001	-0.005
White	-0.001	-0.009	-0.005	-0.003
Total Minority Change	0.009	0.017	0.008	0.010
Total Change	0.005	0.008	0.003	0.006

Note: This table presents the back of the envelope calculations for a simulation of a thought experiment where the racial distribution of teachers matched the racial distribution of students at each school. I calculated the difference in average simulated matches and average observed matches and multiplied those by the point estimate in Table I.5. Panel A shows how many additional or fewer students change their educational attainment from the average simulation. Panel B shows how the race-specific rates change from the average simulation and should be interpreted as a percentage point change in the rate of each race graduating high school or enrolling in college.

Table I.11. Non-Linear Back of Envelope Calculations from Simulations

Panel A: Change from Number of Students				
	HS Grad	Enroll Any	Enroll Two	Enroll Four
Black	27.1	237.6	47.7	278.8
Hispanic	1779.5	5107.1	1958.6	3503.5
Asian	102.3	-161.5	-16.6	-218.5
White	-1215.3	-2170.1	-1343.7	-787.1
Total Minority Change	1908.9	5183.2	1989.7	3563.7
Total Change	693.6	3013.1	646.0	2776.6
Panel B: Change from Rate				
	HS Grad	Enroll Any	Enroll Two	Enroll Four
Black	0.000	0.004	0.001	0.004
Hispanic	0.007	0.021	0.008	0.014
Asian	0.005	-0.007	-0.001	-0.010
White	-0.007	-0.012	-0.008	-0.004
Total Minority Change	0.006	0.015	0.006	0.011
Total Change	0.001	0.006	0.001	0.005

Note: This table presents the back of the envelope calculations for a simulation of a thought experiment where the racial distribution of teachers matched the racial distribution of students at each school. I calculated the difference in the distribution of simulated matches and average observed distribution of matches and multiplied those by the estimates in Tables I.6 and I.7. Panel A shows how many additional or fewer students change their educational attainment from the average simulation. Panel B shows how the race-specific rates change from the average simulation and should be interpreted as a percentage point change in the rate of each race graduating high school or enrolling in college.

Table I.12. Course-level Bounding Set for Estimates

	Test Scores			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match	0.013***	0.0558	X	
Hispanic Match				
Asian Match	0.070***	-0.0263		
White Match				
	Pass Rate			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match	0.0091***	0.008246	X	X
Hispanic Match				
Asian Match	0.014***	0.013051	X	X
White Match				

Note: This table presents the bounding set for each of my significant results in short-term course-level outcomes when accounting for selection of observable and unobservable characteristics using Oster (2019). The first column shows the coefficient I estimate using the full model, and the second column shows the bias-adjusted estimate using Oster (2019). The last two columns evaluate if the bounding set is robust to excluding zero or if the bias-adjusted estimate is within a 99.5% confidence interval of the original estimate. These estimates are plotted in Figure I.13. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table I.13. Individual-level Bounding Set for Estimates

	HS Graduation			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match				
Hispanic Match	0.007***	0.008	X	X
Asian Match	0.018**	0.009	X	X
White Match				
	Enroll Any			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match	0.010***	0.009	X	X
Hispanic Match	0.015***	0.006	X	X
Asian Match				
White Match	0.007***	0.017	X	
	Enroll Two-Year			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match				
Hispanic Match	0.007**	0.001	X	X
Asian Match				
White Match	0.004**	0.011	X	
	Enroll Four-Year			
	Estimate	Bias-Adjusted	Robust to Excluding Zero	Robust to 99.5% CI
Black Match	0.010***	0.005	X	X
Hispanic Match	0.009***	0.005	X	X
Asian Match				
White Match				

Note: This table presents the bounding set for each of my significant results in the linear dosage model when accounting for selection of observable and unobservable characteristics using Oster (2019). The first column shows the coefficient I estimate using the full model, and the second column shows the bias-adjusted estimate using Oster (2019). The last two columns evaluate if the bounding set is robust to excluding zero or if the bias-adjusted estimate is within a 99.5% confidence interval of the original estimate. These estimates are plotted in Figure I.14. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

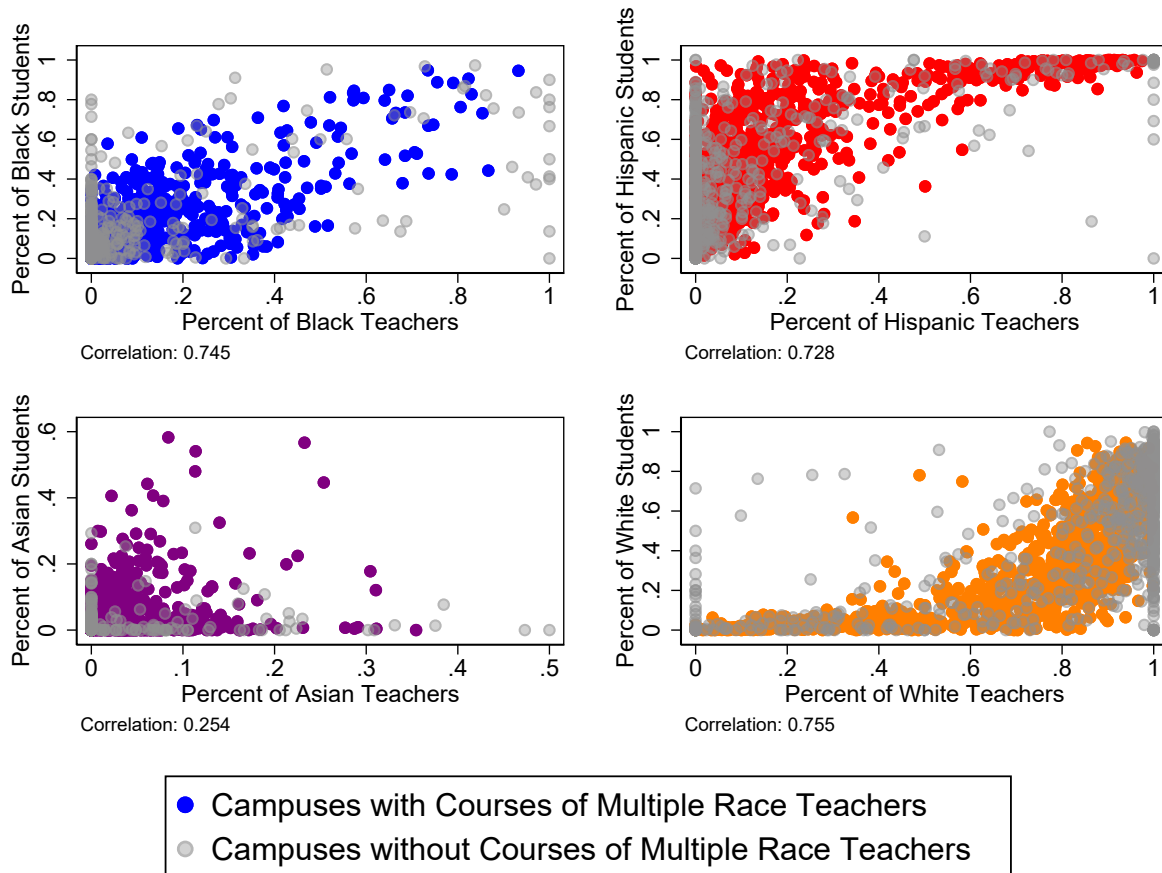
Table I.14. Expanded Course Sets

Panel A: 10th Grade Course Set				
VARIABLES	(1) HS Grad	(2) Enroll Any	(3) Two-Year Enroll	(4) Four-Year Enroll
Black Match	0.003 (0.003)	0.013*** (0.003)	0.005* (0.003)	0.011*** (0.003)
Hispanic Match	0.004* (0.002)	0.018*** (0.003)	0.011*** (0.003)	0.008*** (0.002)
Asian Match	0.007 (0.006)	0.011 (0.011)	0.01 (0.008)	0.008 (0.010)
White Match	0.002 (0.002)	0.005** (0.002)	0.005*** (0.002)	0.001 (0.002)
Observations	407,727	407,727	407,727	407,727
R-squared	0.321	0.286	0.195	0.323
Panel B: 9th and 10th Grade Course Set				
VARIABLES	(1) HS Grad	(2) Enroll Any	(3) Two-Year Enroll	(4) Four-Year Enroll
Black Match	0.001 (0.002)	0.008*** (0.002)	0.0005 (0.002)	0.009*** (0.002)
Hispanic Match	0.002* (0.001)	0.010*** (0.002)	0.007*** (0.002)	0.005*** (0.002)
Asian Match	0.0002 (0.001)	0.003 (0.010)	0.006 (0.009)	0.003 (0.008)
White Match	0.003*** (0.001)	0.004*** (0.001)	0.001 (0.001)	0.003*** (0.001)
Observations	317,979	317,979	317,979	317,979
R-squared	0.564	0.393	0.28	0.392

Note: This table shows the dosage model for 10th grade race matches or 9th and 10th grade race matches conditional on course-set fixed effects. The fixed effects make comparisons between students that took identical courses in the same high school but have different race teachers. The interpretation on the Match coefficient is the effect of one additional same-race teacher. Standard errors are clustered at the high school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

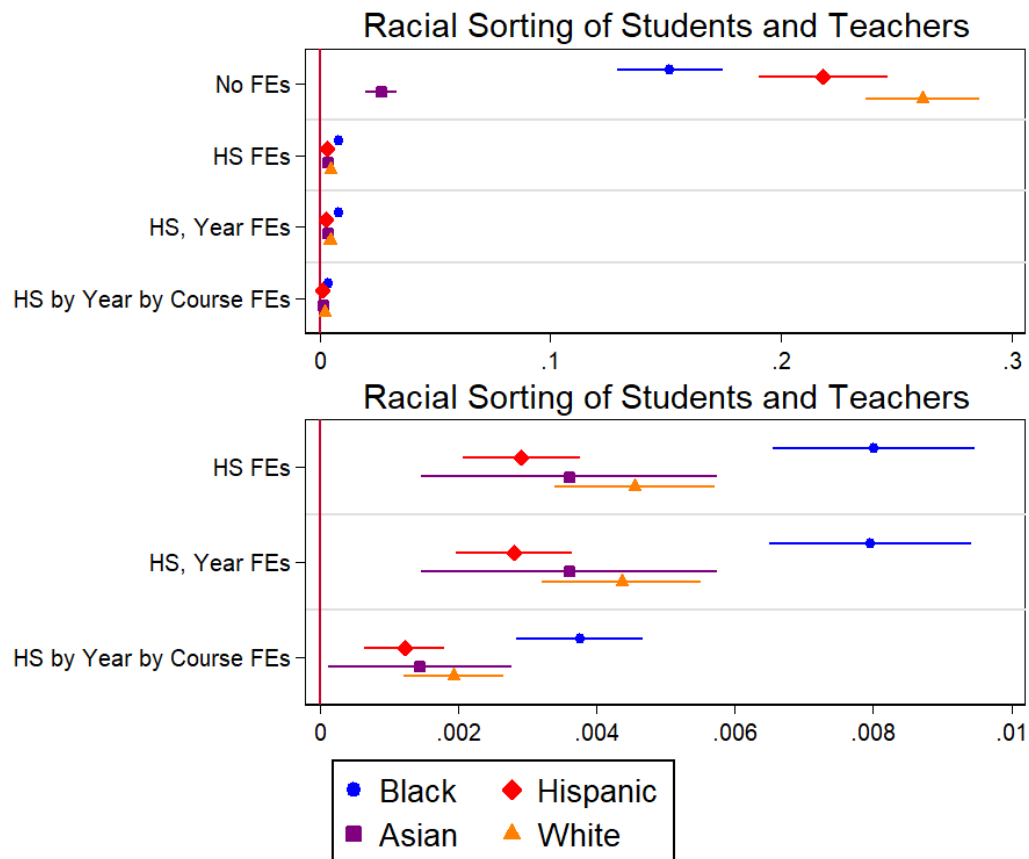
Figures

Figure I.1. Campus Level Identifying Variation



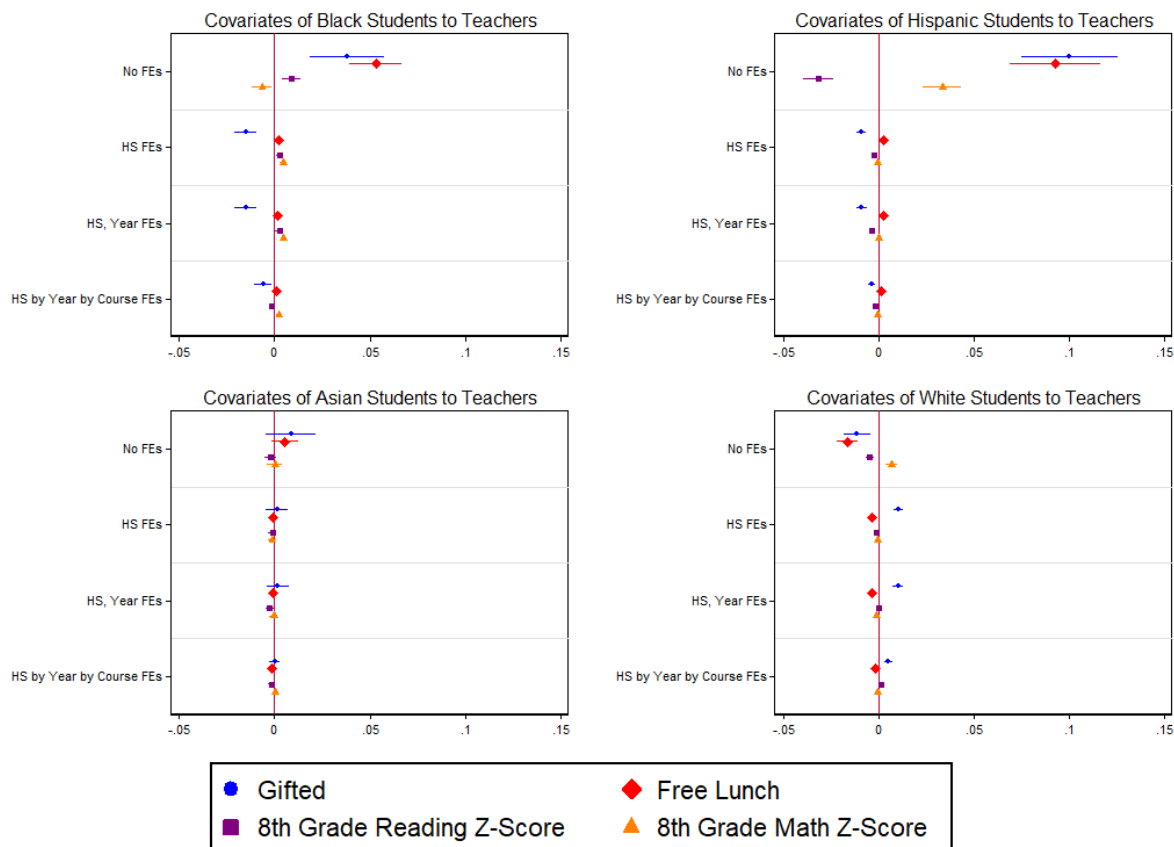
Note: These figures plot the relationship at the campus level between students and teachers of the same race. The top left quadrant shows Black students and teachers, the top right quadrant shows Hispanic students and teachers, the bottom left quadrant shows Asian students and teachers, and the bottom right quadrant shows White students and teachers. The gray dots show campuses that do not contribute to the identifying variation in that they do not have courses with multiple race teachers. The correlation coefficient for each race is given below each plot.

Figure I.2. Likelihood of Same-Race Student Teacher Sorting



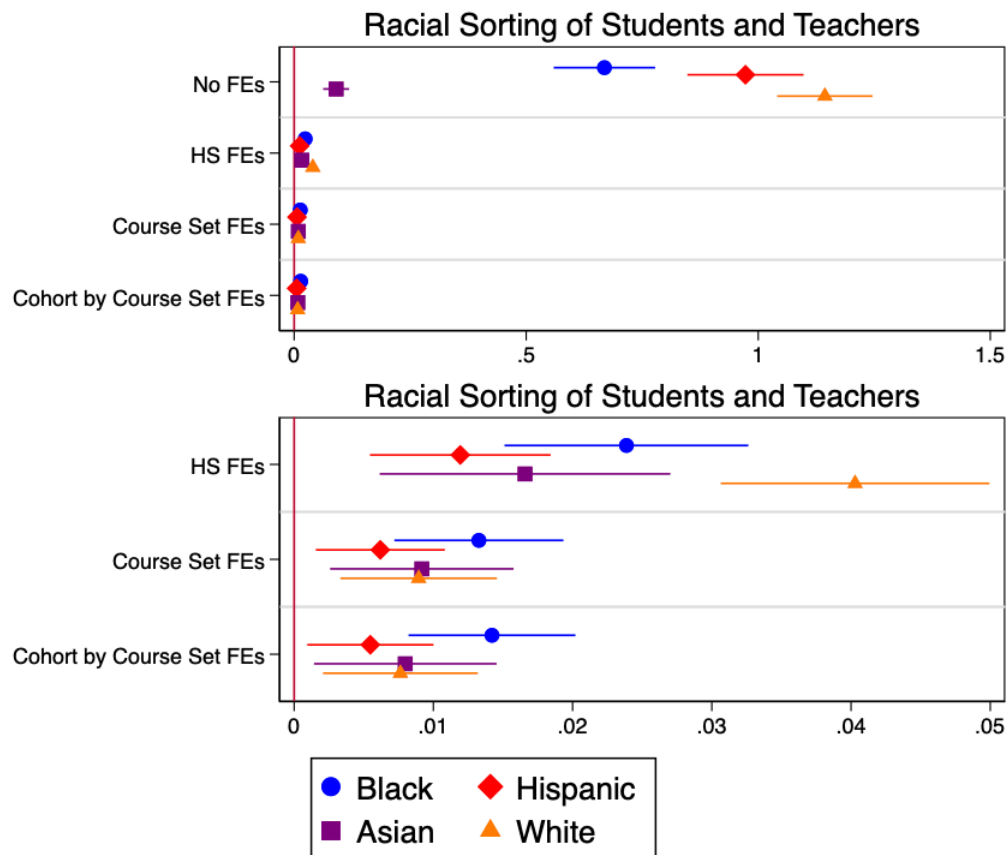
Note: These figures plot the coefficients estimated using regressions I.1-I.4 estimated separately using varying fixed effects to estimate the likelihood that students and teachers are matched along racial lines. The top and bottom figure display the same information and coefficients, but the bottom figure omits the “No FEs” model to show the scaling between the last three models. Racial sorting of students and teachers decreases with high school FEs and decreases further using high school by year by course FEs. Standard errors are clustered at the school level.

Figure I.3. Covariates that Predict Same-Race Student Teacher Assignment



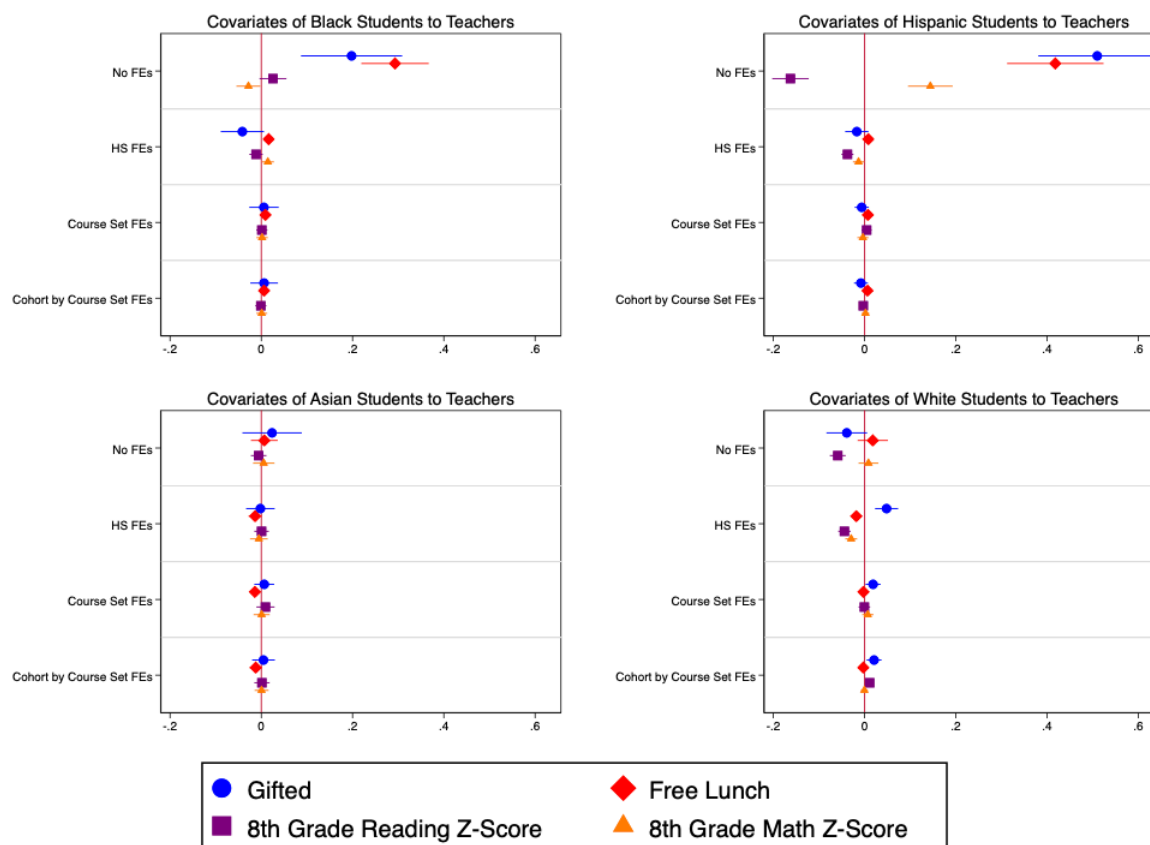
Note: These figures plot the coefficients estimated using regression I.5-I.8 estimated separately using varying fixed effects to estimate what covariates predict students and teachers matching along racial lines. Regressions are limited to one race to examine how the covariates for students of that race predict race matching. Racial sorting of students and teachers decreases with high school FEs and decreases further using high school by year by course FEs. Standard errors are clustered at the school level.

Figure I.4. Dosage – Likelihood of Same-Race Student Teacher Sorting



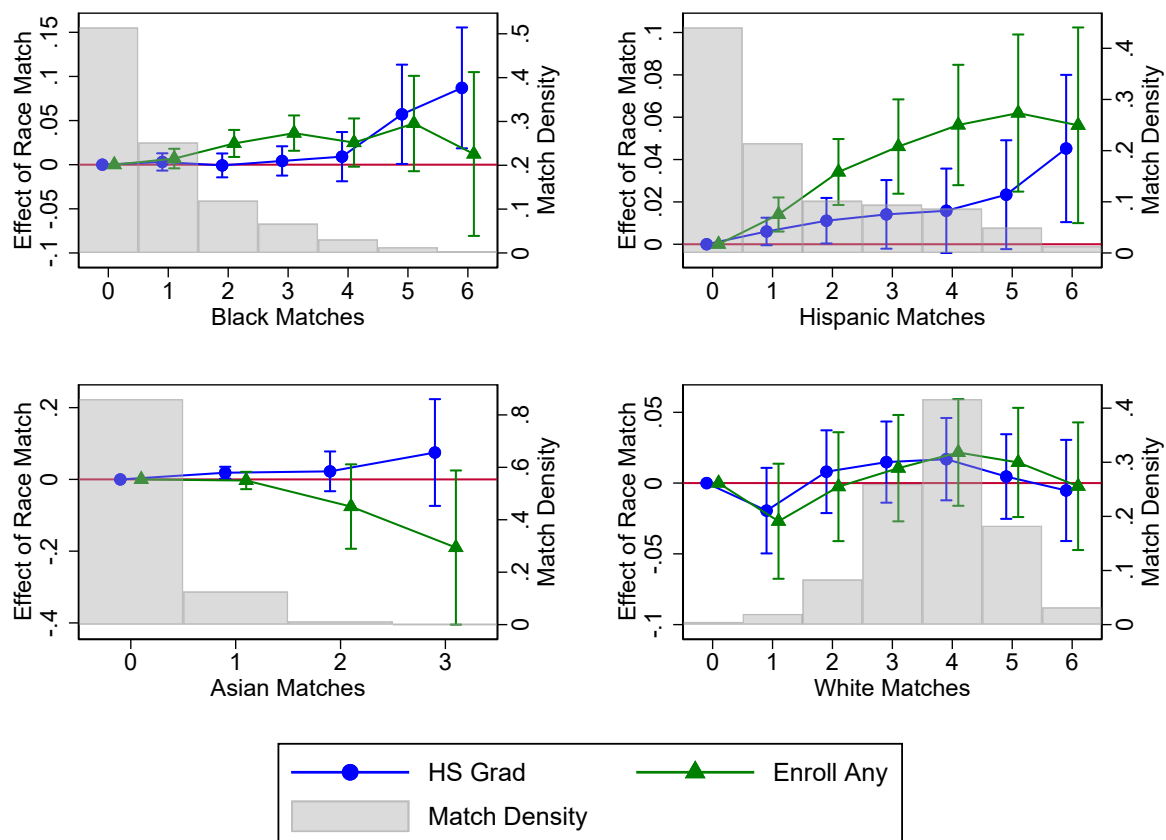
Note: These figures plot the coefficients estimated using regression I.10-I.13 estimated separately using varying fixed effects to estimate the likelihood that students and teachers are matched along racial lines. The top and bottom figure display the same information and coefficients, but the bottom figure omits the “No FEs” model to show the scaling between the last three models. Racial sorting of students and teachers decreases with high school FEs and decreases further using course-set FEs. Standard errors are clustered at the school level.

Figure I.5. Dosage – Covariates that Predict Same-Race Student Teacher Assignment



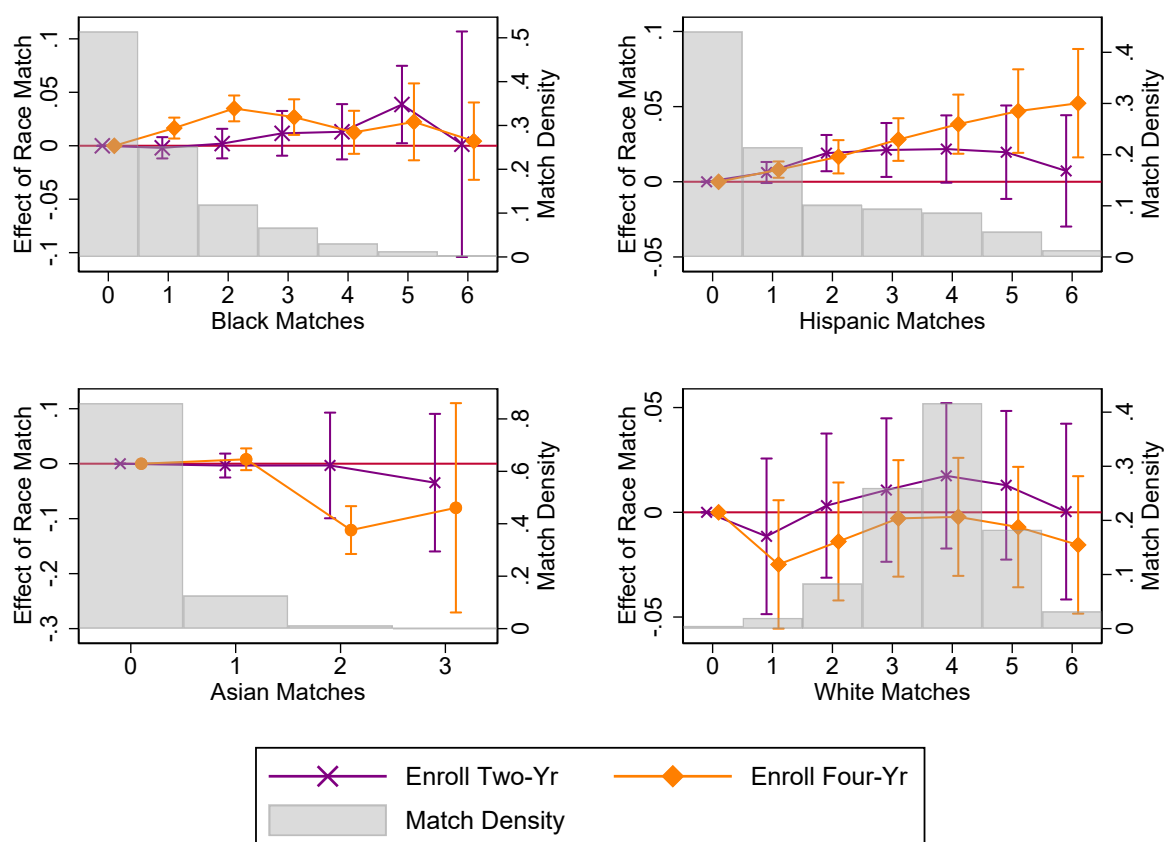
Note: These figures plot the coefficients estimated using regression I.14-I.17 estimated separately using varying fixed effects to estimate what covariates predict students and teachers matching along racial lines. Regressions are limited to one race to examine how the covariates for students of that race predict race matching. Racial sorting of students and teachers decreases with high school FEs and decreases further using course-set FEs. Standard errors are clustered at the school level.

Figure I.6. Non-linear Race Match Effects on HS Graduation and College Enrollment



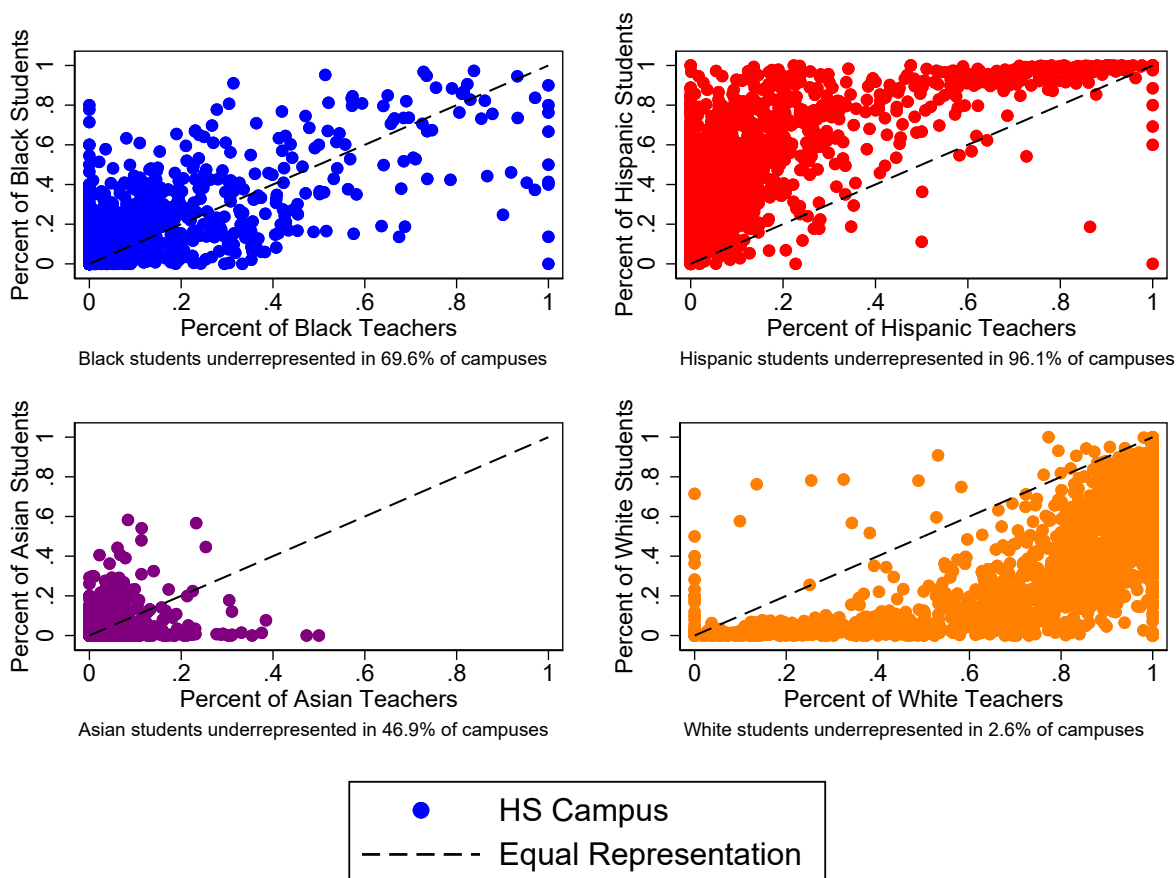
Note: These plots show the non-linear race match effects for high school graduation and college enrollment. The top left quadrant shows the non-linear effects for Black students, the top right quadrant shows the effects for Hispanic students, the bottom left quadrant shows the effects for Asian students, and the bottom right quadrant shows the effects for White students. The group for no race matches is omitted as the reference group. For context, I include the histogram for race-matches for each race in the plot to determine how the support for each plot varies by race. The left y-axis scales the effect size and the right y-axis scales the histogram. The coefficients are displayed in Table I.6. Standard errors are clustered at the school level.

Figure I.7. Non-linear Race Match Effects on Two- vs Four-Year College Enrollment



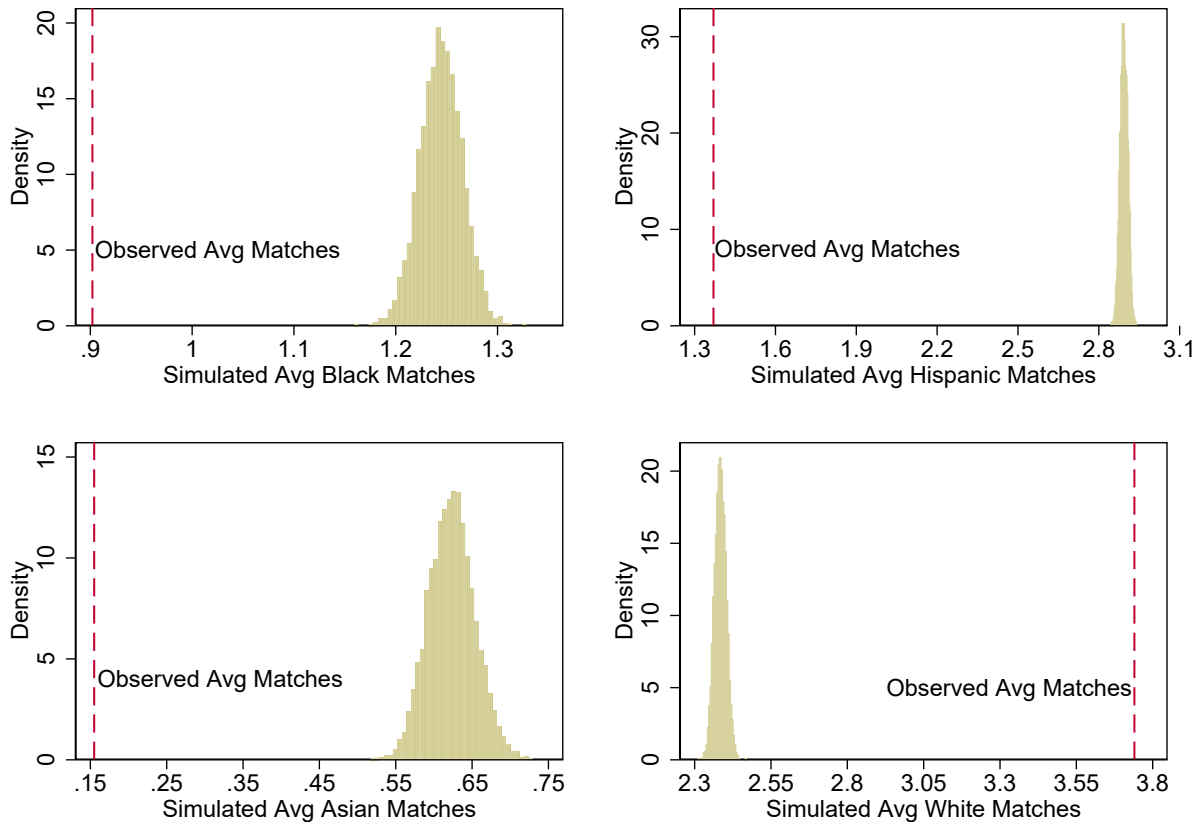
Note: These plots show the non-linear race match effects for two- versus four-year college enrollment. The top left quadrant shows the non-linear effects for Black students, the top right quadrant shows the effects for Hispanic students, the bottom left quadrant shows the effects for Asian students, and the bottom right quadrant shows the effects for White students. For context, I include the histogram for race-matches for each race in the plot to determine how the support for each plot varies by race. The left y-axis scales the effect size and the right y-axis scales the histogram. The group for no race matches is omitted as the reference group. The coefficients are displayed in Table I.7. Standard errors are clustered at the school level.

Figure I.8. Student and Teacher Racial Distribution



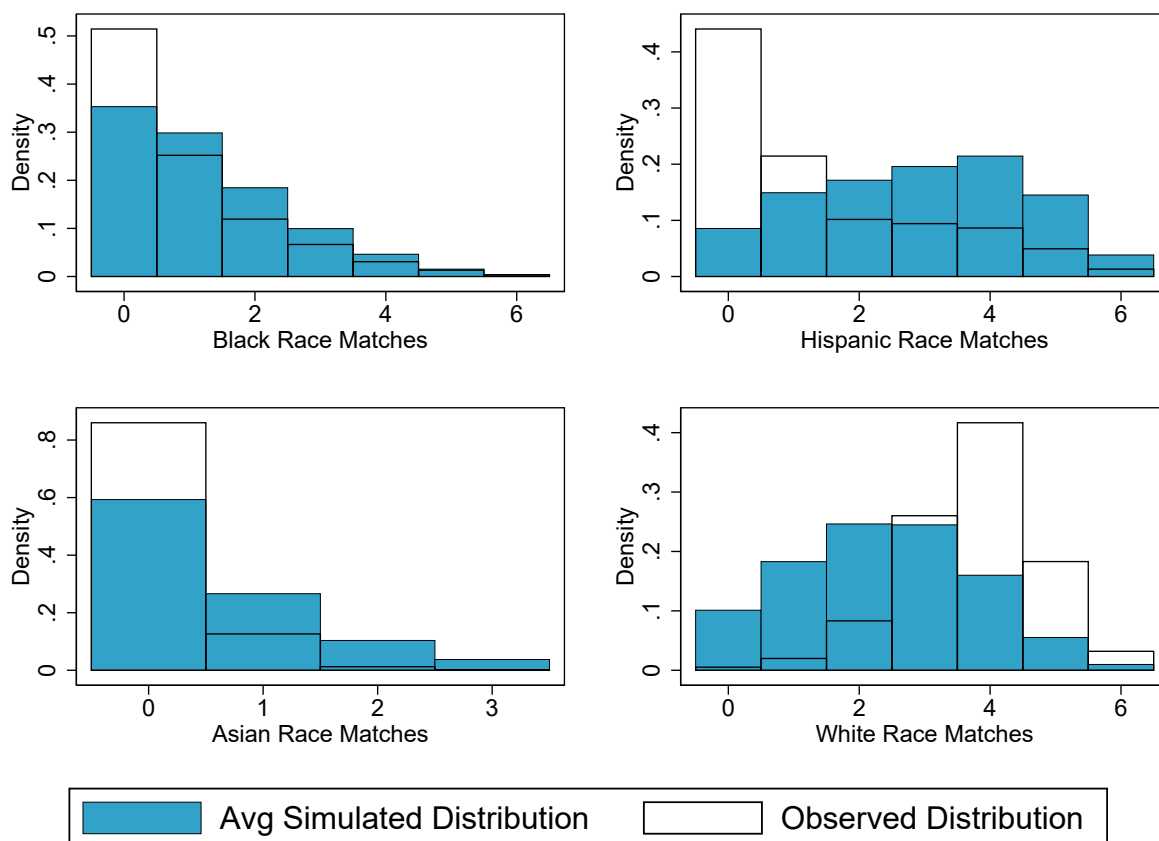
Note: These figures plot the relationship at the campus level between students and teachers of the same race. The top left quadrant shows Black students and teachers, the top right quadrant shows Hispanic students and teachers, the bottom left quadrant shows Asian students and teachers, and the bottom right quadrant shows White students and teachers. The dashed line shows where a school would be if the teaching population were representative of the student population. Being above the dashed line represents students being underrepresented by the teaching population and vice-versa for being under the dashed line. The percentage of campus above the dashed line for each race is given below each plot.

Figure I.9. Simulated Change in Average Race Matches



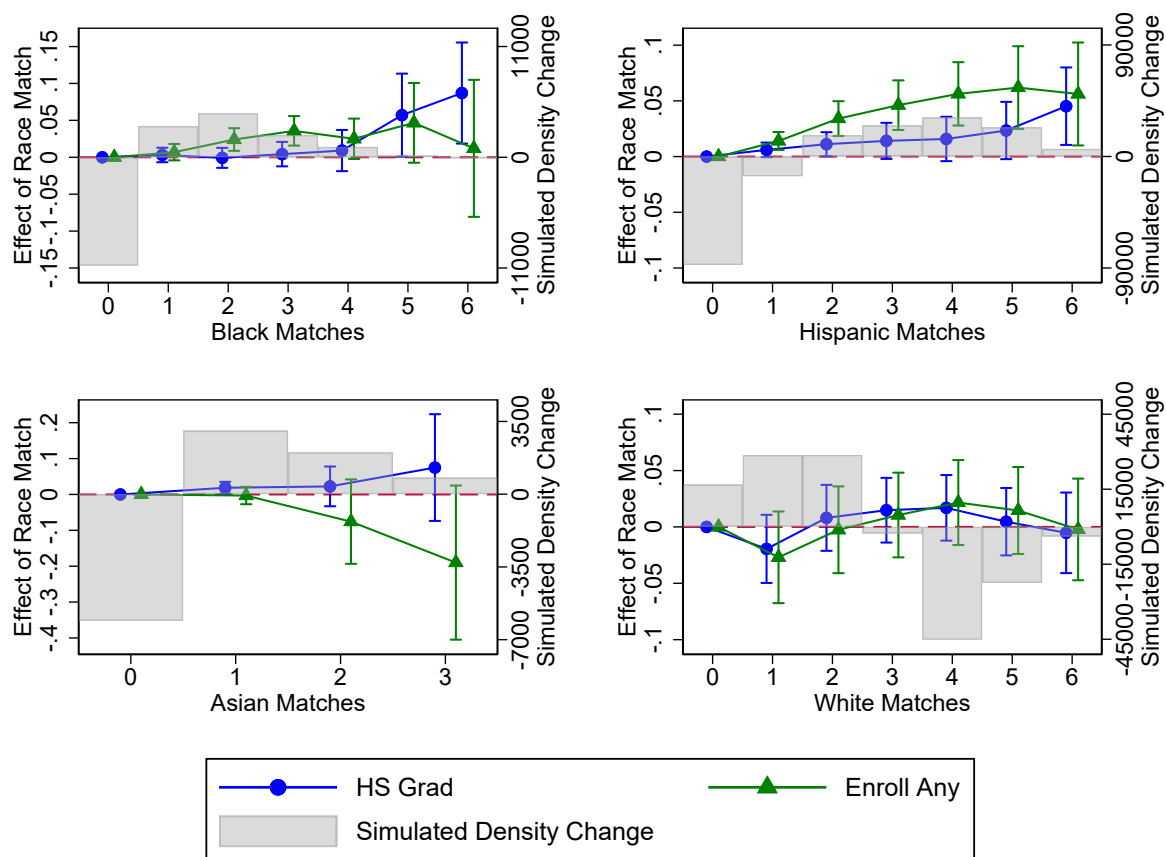
Note: These figures show the distribution of the average number of race matches for 5,000 simulations with the observed number of race matches represented by the dashed red line. The top left quadrant shows Black students, the top right quadrant shows Hispanic students, the bottom left quadrant shows Asian students, and the bottom right quadrant shows White students. In the simulation, there are more Black, Hispanic, and Asian teacher with fewer White teachers compared to the observed matches.

Figure I.10. Simulated Change in Distribution of Race Matches



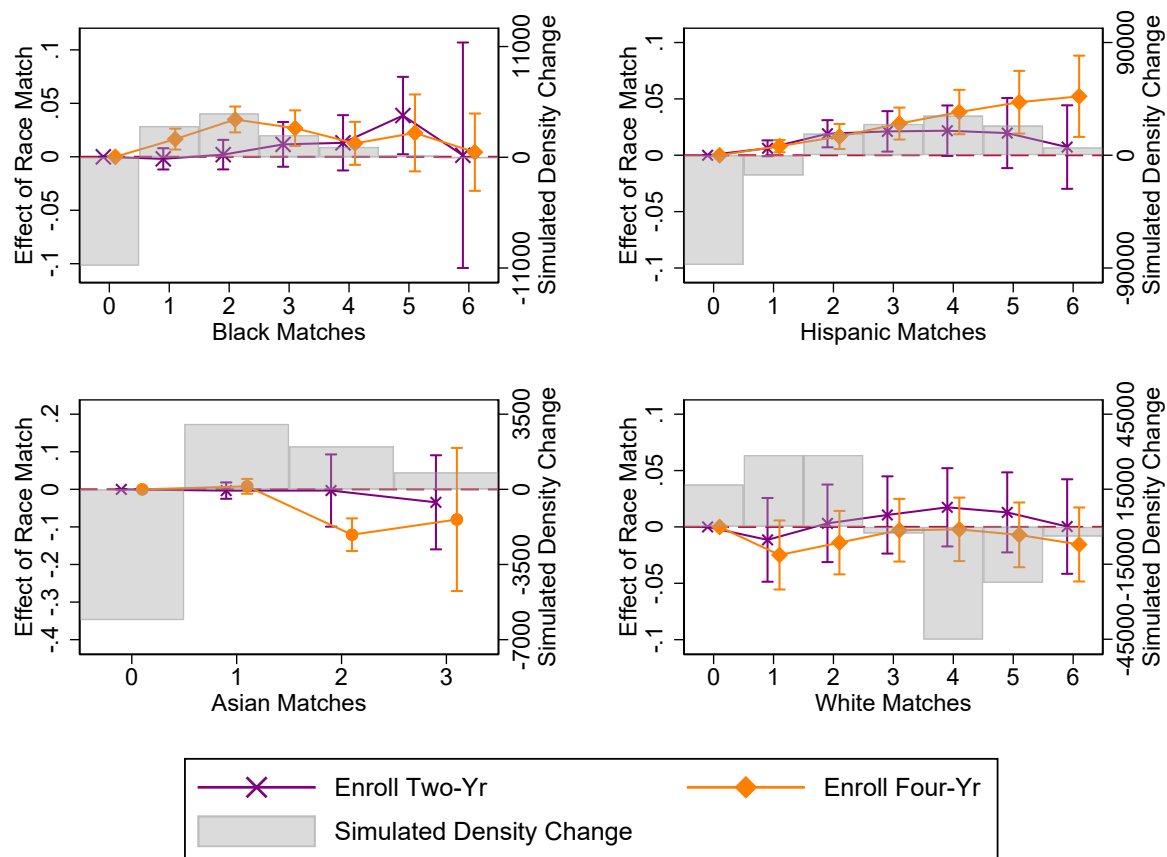
Note: These figures show the change in the average distribution of race matches for 5,000 simulations in the blue bars with the observed distribution of race matches represented by the white bars. The top left quadrant shows Black students, the top right quadrant shows Hispanic students, the bottom left quadrant shows Asian students, and the bottom right quadrant shows White students. In the simulation, there are more Black, Hispanic, and Asian teacher with fewer White teachers compared to the observed matches.

Figure I.11. Simulated Change in Distribution with Non-Linear Effects of Race Matches



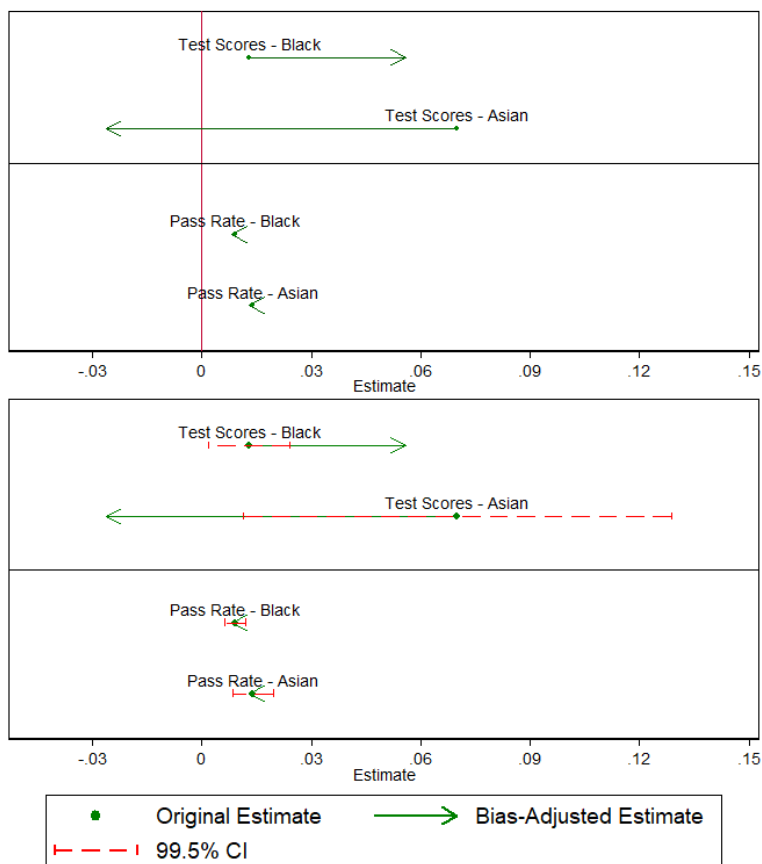
Note: These figures show the change in the average distribution of race matches for 5,000 simulations in the grey bars. The top left quadrant shows Black students, the top right quadrant shows Hispanic students, the bottom left quadrant shows Asian students, and the bottom right quadrant shows White students. The estimates plotted can be seen in Table I.6.

Figure I.12. Simulated Change in Distribution with Non-Linear Effects of Race Matches



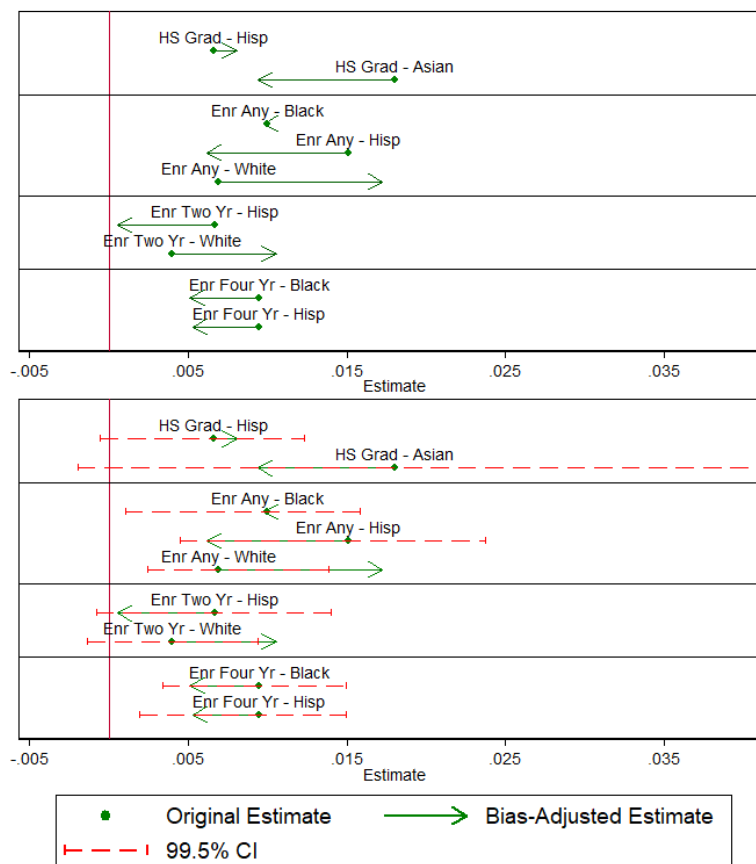
Note: These figures show the change in the average distribution of race matches for 5,000 simulations in the grey bars. The top left quadrant shows Black students, the top right quadrant shows Hispanic students, the bottom left quadrant shows Asian students, and the bottom right quadrant shows White students. The estimates plotted can be seen in Table I.7.

Figure I.13. Bounding Sets and Robustness for Short-term Results



Note: These figures plot the bounding set for significant estimates on race-matching for short-term outcomes. The arrow indicates the direction of the bias and how different the bias adjusted estimate is from the original estimate. The first figure tests if the bounding set includes zero which is shown using a red line. The second figure tests if the bounding set is within the 99.5% confidence interval of the original estimate, denoted using dashed red lines.

Figure I.14. Bounding Sets and Robustness for Long-term Results



Note: These figures plot the bounding set for significant estimates on race-matching for long-term outcomes. The arrow indicates the direction of the bias and how different the bias adjusted estimate is from the original estimate. The first figure tests if the bounding set includes zero which is shown using a red line. The second figure tests if the bounding set is within the 99.5% confidence interval of the original estimate, denoted using dashed red lines.

Chapter II

Sexual Orientation Discrimination in the Workplace

1 Introduction

In 2020, it is legal in 28 states for private businesses to fire an employee for being lesbian, gay, or bisexual (LGB)¹. Unlike race, sex, age, and religion, federal law does not include sexual orientation as a protected class for federal anti-discrimination laws. As a result, laws extending protected class status on the basis of sexual orientation vary between states and localities. The first state to pass an anti-discrimination law on the basis of sexual orientation was Wisconsin in 1982, and the most recent state to pass such a law was Utah in 2015. There has been a renewed push to enact sexual orientation protections at the federal level as Democrats in the House of Representatives introduced “The Equality Act” in early 2019, which would add sexual orientation and gender identity as federally protected characteristics.

I exploit the differential roll out of state and local laws from 2005-2016 in a difference-in-differences framework to analyze how these anti-discrimination laws differentially impact wages and labor supply of LGB workers. The economics literature on LGB workers consistently finds that gay/bisexual men have a pay and labor supply penalty and lesbian/bisexual women have a pay and labor supply premium over their heterosexual counterparts². The differences in pay are attributed to a host of factors including discrimination and intra-household labor allocation and specialization. I test the role of discrimination by examining how the passage of anti-

¹Or for being heterosexual. However, this typically does not happen and is not the focus of this research.

²(Badgett (1995); Klawitter and Flatt (1998); Allegretto and Arthur (2001); Black et al. (2003); Carpenter (2005); Black, Sanders and Taylor (2007); Antecol, Jong and Steinberger (2008); Klawitter (2015); Jepsen and Jepsen (2017); Carpenter and Eppink (2017))

discrimination laws affects the labor supply and wage differences between LGB and heterosexual workers.

There have been previous studies examining how these state and local laws affect the pay gap, but these studies have been limited in their ability to identify causal effects (Klawitter and Flatt (1998); Gates (2009)). Klawitter and Flatt (1998) compared people in same-sex partnerships to those in different-sex partnerships in the 1990 Census and find that same-sex couples have no significant difference in wages in places with anti-discrimination laws. Gates (2009) uses the 2000 Census and conducts a similar analysis. He finds that gay/bisexual men in places with anti-discrimination laws have a 3% wage premium over gay/bisexual men in places without these laws, and lesbian/bisexual women have a 2% wage premium over lesbian/bisexual women in places without these laws³. These past studies on anti-discrimination laws, while informative, fail to fully account for changes over time.

Some previous studies have exploited state-wide sexual orientation anti-discrimination laws in a difference-in-differences framework.⁴ Martell (2013) examines state-wide anti-discrimination laws and shows these laws reduced wage differentials for gay men by 20%. A more recent study by Burn (2018) also looks at state-wide anti-discrimination laws in a difference-in-differences framework, but both studies fail to adequately account for local anti-discrimination laws. I will show empirically that failing to account for local sexual orientation anti-discrimination laws will lead to an estimate biased toward zero since one will mis-assign treatment status without understanding the local context.

This paper is the first to examine both local and state-level sexual orientation anti-discrimination laws in a quasi-experimental design. This paper is also the first to analyze the effects of any local anti-discrimination laws pertaining to sex, race, or sexual orientation in a difference-in-difference framework⁵.

³Tilcsik (2011) found that resumes signalling LGB status received significantly fewer callbacks in localities without sexual orientation anti-discrimination laws.

⁴The Williams Institute found that sexual orientation discrimination occurs at a similar rate to sex-based discrimination using a state-level complaint data following state anti-discrimination laws (Ramos, Badgett and Sears, 2008).

⁵Using state variation in anti-discrimination laws has also been analyzed to understand racial- and sex- based discrimination (Neumark and Stock (2006); (Donohue III and Heckman, 1991);(Margo, 1995); Goldin and Margo

I use the 2005-2016 American Community Survey (ACS) and household composition to infer sexual orientation and create a unique and novel panel dataset on the passage of local and state anti-discrimination laws. I collected information on local laws from a host of sources including media reports, FOIA requests, and an advocacy group. First, I replicate the findings of past research that finds gay/bisexual men have a lower labor force participation and employment rates and make 8-11% less than their employed straight counterparts using hourly wages and annual earnings. I also replicate findings that lesbian/bisexual women have a higher labor force participation and employment rate and earn 5-15% more than their employed straight counterparts.

I find a significant reduction in differences between LGB workers and heterosexual workers across labor supply and wage measures, due to anti-discrimination laws. Anti-discrimination laws significantly reduce the gap in labor force participation and employment of gay men by 1.3 p.p. (18%) and 1.4 p.p. (17%), respectively. The laws also significantly reduce hourly wage gaps between straight and gay men by 2.8 p.p. (11%). The results differ for women, with the laws significantly reducing their labor force participation, employment, and annual wage earnings premium over straight women by 1.3 p.p. (18%), 1 p.p. (14%), and 13 p.p. (16%), respectively. I show using an event study plot that the trends in outcomes are parallel before the implementation of the anti-discrimination laws suggesting the workers in treatment and control regions are reasonable comparisons.

I explore theories for the differing effects of anti-discrimination laws on gay men and lesbian women in the Discussion section, using the Becker (1981) model of household specialization. In the traditional Becker (1981) model of household specialization, men typically specialize in market production, and women typically specialize in household production, in part due to differences in biology where women birth and care for children, resulting in a one-earner household. These differences in household specialization are less pronounced in same-sex partnerships, but they may become more similar for women in same-sex partnerships following the passage of an anti-discrimination law if it gives greater protection to the higher wage earner. I show empirically that the difference in hours worked between partners within a lesbian household

(1992)). However, these studies on sex and race anti-discrimination laws also omit local laws, potentially skewing their estimates.

goes up relative to gay households following anti-discrimination laws, suggesting the lesbian households could become more specialized following anti-discrimination laws with one woman working more hours and the other woman working fewer to focus on household production. I also show that lesbian households have significantly more children than gay households after the passage of an anti-discrimination law. More children could induce lesbian households to further specialize the intrahousehold division of labor, adopting a more traditional household model to help care for additional children.

Finally, my last contribution is the use of polling data on the support for same-sex marriage to examine the relationship between anti-discrimination laws and state sentiment toward LGB workers. One may expect that sentiment toward LGB workers would increase right before the passage of the laws, creating a selection issue. Alternatively, the anti-discrimination laws may normalize being a sexual minority, and improve sentiments toward LGB workers. I collected every poll on same-sex marriage for each state and year from Pew Research Center. I include the polling information to control and proxy for unobservable sentiment toward LGB workers. More importantly, I show that passage of state-wide sexual orientation anti-discrimination laws persistently increases favorability toward LGB people through increased support for same-sex marriage. The states that passed anti-discrimination laws had parallel pre-trends in support for same-sex marriage before the passage of the laws, and afterwards those states had a significant and persistent increase in their support for same-sex marriage. This increase in support following the law passage instead of preceding it, suggests that policy changes may push public opinion instead of vice-versa in contrast to some models of public sentiment like the thermostatic model of Wlezien (1995). Increased favorability toward LGB people may act in conjunction with greater job security to improve labor market outcomes for LGB Americans.

2 Data

A common issue in analyzing the pay gap/premium between homosexual and heterosexual workers is a lack of high-quality data asking about sexual orientation, wages, and employment. I fol-

low the literature (Klawitter and Flatt (1998); Gates (2009); Jepsen and Jepsen (2017)) in inferring sexual orientation by looking at household composition. Specifically, I infer a person's sexual orientation to be homosexual/bisexual if they have an "unmarried partner" or a spouse that is the same sex as themselves⁶. The ACS references a close personal relationship for unmarried partners as well as giving an option for "partner/roommate," which reduces the likelihood that straight roommates would misclassify as unmarried partners.

The comparisons that I make are between people in same-sex relationships, defined as being in an unmarried partnership or married with someone of the same sex, and those in different-sex relationships. This comparison based on household composition, though standard in the literature, is not equivalent to comparing LGB and heterosexual workers. It is comparing those that are in a same-sex relationship to those that are in a different-sex relationship. Notably, this comparison excludes all workers who are not in a cohabitating relationship and classifies bisexual people in different-sex relationships as members of the control group. For example, if a person is misclassified as straight instead of bisexual, and bisexual workers have a lower pay than their straight counterparts, then the misclassification would bias our pay gap/premium results toward zero.

I use the 2005-2016 yearly ACS from IPUMS USA (Ruggles, et al 2019). I use wages, defined as salaried wages from an employer. The ACS reports wage earnings in annual terms. I use the annual figures and convert them to hourly wage figures using variables on average weekly hours worked and weeks worked⁷. Finally, I limit my sample to prime-age working adults and only examine those who are ages 25-65. I use 25 as the lower age cutoff to allow workers to complete college and have more fully developed human capital, which is typically thought of as an important factor for the differences in wages between LGB and straight workers (Black, Sanders and Taylor, 2007).

I obtained data on the passage of state anti-discrimination laws from LGBTMap.org, an LGBT advocacy group. The website gives information on which states passed anti-discrimination laws

⁶People with imputed sex value are dropped from the sample.

⁷"Weeks worked" in the ACS is a categorical variable giving a range of weeks worked. I take the median value given in the range to compute weeks worked.

and when. I focus solely on sexual orientation anti-discrimination laws that give protection in employment. LGBTMap.org also provided incomplete data detailing the passage of local city and county laws with many cities missing years for the start of their anti-discrimination laws. I completed the dataset for the relevant years using old media reports, correspondence with local officials, and FOIA requests. This dataset is the first complete city level dataset on sexual orientation anti-discrimination laws in the U.S. I focus on cities reported in the ACS and matched those city laws with their corresponding counties to merge in with the ACS. The ACS only reports county of residence for those in metro areas, so any rural counties are lumped together. I make the assumption in my analysis that the impact of these laws is the same across states and localities, estimating an average effect.

I report the state, city, and county laws and the year they were enacted in Table II.1. In Figures II.1 and II.2, I show which counties had sexual orientation anti-discrimination laws in 2005 and 2016, respectively.

Enactment of anti-discrimination legislation is not random. The states that have these protections are generally considered friendlier to LGB workers than those without these laws and have a larger share of same-sex partnerships than those states without these laws. Also, many of the counties that have anti-discrimination laws have large cities that have a larger concentration of LGB workers than rural counties. However, this is not universal. For instance, Utah extended protection to LGB workers despite being a relatively conservative state, and certain liberal cities like Houston⁸ are noticeably absent from extending protection to LGB workers. One potential concern in this analysis is the endogenous adoption of laws. The areas that are friendly and less hostile to LGB workers may be the areas that are most likely to adopt sexual orientation anti-discrimination laws. The levels of sentiments and friendliness to LGB workers is not the concern, as a county-fixed effect will compare a given county to itself. The concern is that the timing of anti-discrimination laws is correlated with another factor causing an omitted variable bias. It could be that positive sentiment toward LGB workers cause both the law change and any change in labor market outcomes. I attempt to control for sentiment toward LGB workers in a given

⁸Houston passed a sexual orientation and gender identity anti-discrimination ordinance, but it was only in effect for 3 months before being challenged. The law was put up to a public vote and lost, repealing the law.

state at a given year by including polling information on support for same-sex marriages. Unfortunately, I cannot observe polling at a local level, as such the polling measure is an incomplete proxy for sentiment toward LGB workers.

I collected every poll that Pew Research Center has published from 2005-2016 to pull each poll regarding support for same-sex marriage. The 28 polls asking about same-sex marriage were aggregated to get the percentage of people that supported same-sex marriage by state by year. The polling information contains data on every state for every year, except for Alaska and Hawaii, which are missing polling information for 2005-2008.

In Figure II.3, I present the distribution of same-sex partnerships by state from 2005 to 2016, and in Table II.2, I present the 10 counties with the largest share of same-sex partnerships.

Unsurprisingly, the distribution of same-sex partnerships by state and by county are skewed toward more progressive states and counties with large cities in them that are known for having a large LGB population like San Francisco, the District of Columbia, New York City, and Boston. The LGB population varies from the straight population in many dimensions including geography. I present descriptive statistics showing the differences in the LGB population compared to the straight population broken down by education level and sex in Table II.3. Descriptively, there is a clear difference in labor market outcomes and characteristics between men and women in same-sex partnerships and those in different-sex partnerships when controlling for education.

3 Identification Strategy & Estimation

The differential rollout of anti-discrimination laws by state and locality over time lends itself to a difference-in-differences framework with the main outcomes of interest as the change in labor supply and pay between two groups, LGB workers and heterosexual workers. This strategy uses the variation presented in Figures II.1 and II.2, which shows how the laws changed over time by state and county.

Formally, I estimate this equation on labor supply:

$$LS_{ict} = \alpha_0 Law_{ct} + \alpha_1 SSP_i + \alpha_2 Law_{ct} * SSP_i + \alpha_3 X_i + \alpha_4 \gamma_{st} + \mu_c + \delta_t + \epsilon_{itcs} \quad (II.1)$$

Where LS_{ict} is the labor force participation or employment status for person i in county c in year t . Law_{ct} is an indicator for if county c has an employment anti-discrimination law on sexual orientation or is covered by a state-wide anti-discrimination law in year t , and SSP_{it} is an indicator for if person i is in a same-sex partnership. X_i is a vector of person-specific covariates including education, age, race, children, etc. γ_{st} is a vector for state s covariates on the legality of same-sex marriage in year t as well as polling for the support of same-sex marriage. It includes the percentage of people that “strongly support,” “support,” “oppose,” and “strongly oppose” same-sex marriage in a given state in a given year. Finally, μ_c gives county fixed effects, δ_t gives year fixed effects, and ϵ_{itcs} is the error term⁹. My sample is limited to people aged 25 to 65, and I estimate this model separately by sex using the ACS person weights. I cluster the standard errors at the county level, as that is the level of the treatment in this specification. The coefficients of interest are α_2 , which will give the effect of the law on the labor supply gap/premium, and α_1 , which gives the labor supply gap/premium in the absence of an anti-discrimination law. My specification makes the assumption that the effect of the laws is the same across states and localities, and I will be estimating an average effect.

I estimate a similar equation for wages:

$$\ln(Y_{ict} + 1) = \beta_0 Law_{ct} + \beta_1 SSP_i + \beta_2 Law_{ct} * SSP_i + \beta_3 X_i + \beta_4 \gamma_{st} + \mu_c + \delta_t + \epsilon_{itcs} \quad (II.2)$$

Where Y_{ict} is the real value, in 1999 dollars, of person i 's wages in county c year t . The rest of the notation is the same as above. The coefficients of interest are β_2 , which will give the effect of the law on the pay gap/premium, and β_1 , which gives the pay gap/premium.

One concern is that the distribution of LGB workers is not random, and states/counties that pass anti-discrimination laws have a higher concentration of LGB workers. However, the use of county fixed effects will make comparisons within a given county, so San Francisco will be

⁹Since the ACS only identifies metro counties, the county fixed effect is close to a pseudo-county fixed effect. The rural counties are all compared together with a true county fixed effect for metro area counties.

compared to San Francisco. A potential concern would be if there is sorting of LGB workers to areas that have anti-discrimination laws protecting them, changing the composition of the area. I show that there is little evidence to support this in the robustness check section.

My identifying assumption in this difference-in-differences framework is that the labor supply and wage gap/premium in counties that have anti-discrimination laws would have continued parallel with the counties that did not have anti-discrimination laws in the absence of the laws. While the parallel trends assumption of difference-in-differences methodology is inherently untestable, it is common to see how parallel the pre-trends are before each state or county receives the treatment to places that are untreated. I examine the outcomes relative to the timing of the laws by estimating an event study model:

$$\begin{aligned}
 LS_{ict} = & \sum_{j=-5, j \neq -1}^{j=5} \rho_j \mathbb{1}(YearsWithLaw_{ct} = j) + \\
 & \sum_{j=-5, j \neq -1}^{j=5} \tau_j \mathbb{1}(YearsWithLaw_{ct} = j) * SSP_i + \\
 & \theta_1 SSP_i + \theta_2 X_i + \theta_3 \gamma_{st} + \mu_c + \delta_t + \epsilon_{itcs}
 \end{aligned} \tag{II.3}$$

I estimate this equation for labor supply measures and wage measures where $\mathbb{1}(YearsWithLaw_{ct} = j)$ is an indicator for if county c has had the law for j years in time t . I plot the τ_j s, which give the effect of the law on the labor supply and pay gap/premium in each year relative to the start date. The effect of the law in the year before the law is enacted is normalized to zero.

4 Results

4.1 Main Results

In Table II.4, I present the regression on the extensive margin of labor supply with Panel A giving the effect of anti-discrimination laws on gay men's labor supply and Panel B giving the effect for lesbian women. I present the full specification in columns (1) and (2) and include a specification without accounting for local anti-discrimination laws in columns (3) and (4) to show how failing to account for local laws would lead to the incorrect inference.

Consistent with the previous literature, I find that lesbian women have a labor supply premium of and gay men have a labor supply penalty on the extensive margin. Lesbian women are 7.3% more likely to be employed and gay men are 7.9% less likely to be employed than their straight peers. Anti-discrimination laws have a significant effect in reducing the labor force participation and employment gap for gay men. The labor force participation rate gap is reduced by 1.3 p.p. (18%), and the employment gap is reduced by 1.4 p.p. (18%). Conversely, the labor force participation and employment premium that lesbian women have over their straight counterparts is reduced by 1.3 p.p. (16%) and 1 p.p. (14%), respectively. As seen in columns (3) and (4) of Panel A, estimating the effect of anti-discrimination laws only using state laws will lead one to erroneously conclude that these laws have no significant effect on the gay labor force and employment gap. However, the effect on the extensive margin of labor supply for lesbian women looks similar when estimating the effect just state anti-discrimination laws.

In Table II.5, I present the regression results on the intensive margin of labor supply as measured by weekly hours worked and weeks worked. Similar to Table 4, I include the results when only estimating the effects of the state laws in columns (3) and (4) to show the contribution of including local laws in estimating the effect of anti-discrimination laws.

I replicate past findings where gay men supply significantly less labor and lesbian women supply significantly more labor on the intensive margin. As with Table II.4, I find contrasting effects of anti-discrimination laws on male and female outcomes. I find that the intensive labor supply gap for gay men as measured by weekly hour and weeks worked is significantly reduced, and the intensive labor supply premium for lesbian women is significantly reduced as well. The effect of anti-discrimination laws on weekly hours worked becomes insignificant for gay men and lesbian women when only estimating off of state-wide anti-discrimination laws.

In Table II.6, I present the regression results on the wage gap as measured by hourly wages and annual wage earnings. These variables received a $\log(x+1)$ transformation, to handle zero values and so the changes can be interpreted in percentage terms. I run the regressions separately by sex and present the regressions showing how the sexual orientation anti-discrimination laws impact both employed and unemployed people in columns (1) and (2) and limit the regressions to only

those who are employed in columns (3) and (4). My preferred specification is to look at both employed and unemployed people in the sample as employment is endogenous. Conditioning on employed workers would create an endogeneity problem since labor force participation and employment are significantly impacted by anti-discrimination laws. Given Table II.4, the sample of employed workers could be changing as a result of the law changes, which would lead to a change in the sample composition. I present the regressions limited to employed workers to replicate past work on the wage gap/premium where it is common to only examine employed workers. I include results with only using state anti-discrimination laws, excluding local laws. I present those results in columns (5) - (8).

Gay men see a significant decline in the hourly wage gap by 2.8 p.p. (11%) at the 0.1 significance level and see an insignificant decline in the annual earnings gap by 5.8 p.p. On the other hand, lesbian women see a significant change in their annual earnings premium by 13 p.p. (16%) and an insignificant reduction in their hourly wage premium. Columns (3) and (4) replicate the past literature where employed gay men make about 8% to 11% less than their straight counterparts and employed lesbian women make about 5% to 15% more than their straight counterparts.

The pay gap/premium is reduced quite significantly once the sample is limited to employed workers, which is unsurprising. As shown in the Table II.4 and Table II.5, there are significant differences in labor supply for LGB and straight workers. Gay men and straight women are far more likely to be unemployed, out of the labor force, and work fewer hours and weeks, so including their zero wages in the regression will increase the gap/premium.

There is a significant decrease in the hourly wage gap now and an insignificant decrease in the annual wage gap. There is also a significant decrease in the annual earnings premium for lesbian women but no change in their hourly wage premium. This result is entirely consistent with the labor supply results in Table II.4 and II.5 where their intensive and extensive margin of labor supply premium are declining, so the annual earnings declines, but hourly wage does not.

I present the event study regressions for male and female extensive labor supply outcomes in Figure II.4 with the male results in the top row and the female results in the bottom row. There is an initial uptick in the relative employment and labor force participation rate for gay men that

diminishes after a few years but reappears at the end. These graphs suggest an initial positive effect that is tempered in the medium-term but could persist in the long-term. The point estimates are smaller and insignificant but remain positive after implementation, which could be caused by the reduced sample size that occurs from slicing the data into yearly bins. The pre-trends before the treatment look fairly parallel though for the point estimates. Pre-trends being parallel does not validate the identifying assumption of parallel trends in the post-period, but it makes the assumption more palatable. These figures suggest that there is a temporary boost to gay labor force participation/employment relative to straight employment/labor force participation. In the second row, there appears to be continuous downward trend in the point estimates for the labor force participation gap irrespective of the enactment of these anti-discrimination laws, although employment looks relatively stable in the pre-trends. The confidence intervals for the pre-trends encapsulate zero though.

I present the event study regressions for male and female pay penalty/premium outcomes in Figure II.5, with the male results in the top row and the female results in the bottom row. The pre-trends look parallel before the treatment on the gay hourly wage and annual earnings gap. It appears that it takes time though for the gay workers to see gains in their relative wages to straight workers, but there appears to be a temporary spike in relative gay wages in time period 1 after law passage, that goes away after a short time. The spike could occur in time period 1 after law passage due to implementation lags. The event study plots for lesbian women looks less parallel in the pre-period and appear to be downward trending for the point estimates. However, the confidence intervals encapsulate zero for all of the pre-periods. There is a significant drop in relative lesbian wages in time period 2 after the law passage.

4.2 Occupation Fixed Effects

LGB workers tend to sort to different occupations and industries based on sex and sexual orientation (Black, Sanders and Taylor, 2007). They show that gay men tend to sort to more female dominated fields and lesbian women sort to more male dominated fields relative to their heterosexual counterparts. It is typical to not take those factors into consideration because discrimina-

tion affects occupation and industry choice, which would make occupation and industry choice endogenous.

While controlling for occupation will bias the wage gap, it can still be instructive for seeing the effect of anti-discrimination laws of the wage and labor supply gap. I run these regressions with occupation fixed effects using four-digit occupation codes in the ACS to see how the pay gap/premium changes after anti-discrimination laws are passed. Using occupation fixed effects will examine the effect of anti-discrimination laws within an occupation. One potential mechanism for anti-discrimination laws is that gay workers sort into more in-demand occupations, lowering their relative labor supply and wage gap, while lesbian workers do the opposite. However, I show that even when controlling for occupation that there is a significant change in relative labor supply for men and women in same-sex partnerships and a significant change in relative pay for women in same-sex partnerships, indicating there is a significant effect of these laws even within occupation. I present the extensive labor supply and pay regressions with occupation fixed effects in Table II.7 and II.8.

In these tables, there is a smaller pay gap for gay men and a smaller pay premium for lesbian women when controlling for occupation or industry. However, qualitatively the results seem to be similar to the main results in Table II.4 and Table II.6 . Gay men see a significant reduction in the labor supply gap, and lesbian women see a significant reduction in their labor supply and annual wage premium. There is no longer a significant effect on the wage gap for gay men. These findings suggest that anti-discrimination laws have a significant effect even within occupations. The effect does not come from workers shifting into occupations with less discrimination, but even when conditional on occupation choice, there is a significant reduction in differences of labor supply across sexual orientations for men and women.

5 Robustness Checks

5.1 Endogenous Adoption of Anti-Discrimination Laws

One concern in this analysis is the endogenous adoption of anti-discrimination laws. Clearly, anti-discrimination laws are not randomly distributed. Locally, anti-discrimination laws are concentrated in larger cities, and state laws are to be concentrated in more liberal states that presumably are more accepting of and more favorable to LGB workers. The main concern is that there is an unobservable factor like general sentiment toward LGB workers that affects both the passage of laws as well as the labor market outcomes for LGB workers.

In my main regressions, I control for this concern by using state-level polling information on support for same-sex marriage as a proxy for general sentiment toward LGB workers. It is possible that controlling for state-level polling is not the best way to capture sentiment toward LGB workers since it is possible to discriminate against people based on sexual orientation and still support their right to marry. However, it seems plausible that the changes in state-level support for same-sex marriage are highly correlated with changes in sentiment toward LGB workers such that it will suffice for a suitable proxy. An important limitation to this analysis is that I am unable to see local-level polling information on support for same-sex marriage. Local-level sentiment toward LGB workers is unobservable. My regressions include state-level polling so any effect identified from law changes is conditional on the state-level information, but it is not conditional on local-level sentiment.

To better get at the question of endogenous adoption of laws, I create an event-study plot showing how state-laws change support for same-sex marriages. Specifically, I estimate this equation:

$$Support_{st} = \sum_{j=-5, j \neq -1}^{j=5} \psi_j \mathbb{1}(YearsWithLaw_{st} = j) + \phi_s + \delta_t + \epsilon_{ts} \quad (\text{II.4})$$

$Support_{st}$ gives the support for same-sex marriage in state s in year t , and $\mathbb{1}(YearsWithLaw_{st} = j)$ is an indicator for if state s has had a sexual orientation anti-discrimination law for j years in time t . I plot the ψ_j s, which give the effect of anti-discrimination laws on the support for

same-sex marriage. This estimation strategy is an event study plot in a typical difference-in-differences set up. The identifying assumption is that support for same-sex marriage in states that passed anti-discrimination laws would have continued in parallel with states that did not pass anti-discrimination laws. I present the event study in Figure II.6.

The event study supports the idea that anti-discrimination laws at the state-level significantly increase the support for same-sex marriage at the state-level. The pre-trends are relatively parallel with a significant and persistent increase in the percentage of people supporting same-sex marriage, suggesting that states with and without anti-discrimination laws had the same trends in support for same-sex marriage before laws and differed once the laws passed. It could be that the laws passed due to the change in support for same-sex marriage at the moment of the law passage, but endogenous adoption of the laws would likely show that there is a continuous increase in the support for same-sex marriage before the adoption of the law with the law having no effect on the support for same-sex marriage. However, the persistent and significant jump right in conjunction with the passage of anti-discrimination laws gives more credence to the hypothesis that the law changes increases support for same-sex marriage and likely overall sentiment to LGB workers rather than vice-versa.

My results condition on the state-level change in polling in the support for same-sex marriage and condition on this jump in sentiment that occurs following a state law change. However, they do not condition on local level changes in support for same-sex marriage. I showed previously in the paper that the local laws matter significantly for the correct inference of passing anti-discrimination laws. The jump seen in state-level sentiment toward LGB workers following state anti-discrimination law passages could suggest that there is a corresponding jump in sentiment at the local level that is unobservable and potentially acting as a mechanism, affecting the labor supply and pay for LGB workers.

5.2 Sorting and Increased Reporting

One finding from Klawitter and Flatt (1998) suggests that LGB workers sort to areas with anti-discrimination laws. If LGB workers were sorting to areas that recently passed anti-discrimination

laws, then it could violate the identifying assumption. It would be concerning if high-wage LGB workers migrated from unprotected areas to areas that recently passed anti-discrimination laws since it would lower the average wage in untreated counties and increase the average wage in treated counties.

Another potential concern would be increased reporting for same-sex partnerships. Same-sex partnerships, while less stigmatized in 2005-2016 than in previous years, were still heavily stigmatized. Anti-discrimination laws could signal to LGB people that their community was more accepting, and people may be more likely to declare that they are in a same-sex partnership. This change in reporting could affect the composition of my sample, which in turn could violate the identifying assumption.

To address this concern, I run the regression below to see if there is an increase in same-sex partnerships in a given county after an anti-discrimination law is passed:

$$SSP_{ict} = \beta_0 Law_{ct} + \beta_3 X_i + \beta_4 \gamma_{st} + \mu_c + \delta_t + \epsilon_{itcs} \quad (II.5)$$

In this regression, β_0 is the coefficient of interest, and it will give the effect of anti-discrimination laws passing on the number of same-sex partnerships in a given county whether that is from immigration or increased reporting of same-sex partnerships. I present these results in Table II.9.

I find no significant effect of anti-discrimination laws on the number of same-sex partnerships at the county level, suggesting there is minimal sorting or change in reporting following the passage of anti-discrimination laws.

6 Discussion

6.1 Differences in Response by Sex

The results paint an interesting picture of the effect of anti-discrimination laws on the workforce, in that they have differing effects on lesbian and gay workers. In summation, lesbian workers see a reduction in the labor supply and pay premium while gay workers see a reduction in their labor supply and pay gap. The anti-discrimination laws appear to push the labor market outcome to a greater level of equality across sexual orientation, where lesbian women move toward the

labor market outcomes of straight women and gay men move toward the labor market outcomes of straight men.

One potential explanation for this convergence of labor market outcomes is that employers stop differentiating between workers based on sexual orientation after anti-discrimination laws are passed and that results in greater equality. However, much of the labor market differences arise from differences in human capital accumulation and decisions that come before many of the anti-discrimination laws such as occupation/industry choice and desired number of children (Black, Sanders and Taylor, 2007).

Another explanation for different responses to anti-discrimination laws for same-sex couples lies in the canonical Becker (1981) theory of the family and division of labor, which is typically used to explain differences in LGB labor market outcomes from their straight counterparts (Black, Sanders and Taylor, 2007). In a male-female household with a “traditional” division of labor, the man specializes in market production, selling his labor for money to buy market goods, and the woman specializes in household production, using her labor to rear children, cook, and take care of the home, etc. However, LGB people are less likely to end up in a traditional Beckerian household. This expectation of future household composition will induce gay men to be less specialized in market production and lesbian women to be more specialized in market production than their straight counterparts. This theory of household specialization is typically used to explain why gay men make less than straight men and lesbian women make more than straight women (Black, Sanders and Taylor, 2007).

This household model can also be used to think about responses to anti-discrimination laws. One potential explanation for lesbian women becoming differentially more likely to leave the workforce and earn less is that a lesbian household may be more likely to shift to a more traditional Beckerian household where one parent specializes in household production and the other shifts to a more market orientation. If anti-discrimination laws decreased the risk of the primary earner getting fired for their sexual orientation, it could induce the secondary earner to switch to more part-time work or exit the workforce to specialize in household production or focus on child rearing. Each woman would specialize in her intrahousehold comparative advantage. Alterna-

tively, both women in a female same-sex partnership could switch to reducing hours to spend more time in household production as a result of increased job security.

Households consisting of lesbian women typically have more children than households consisting of gay men, and much of the gains from task specialization come from children. Given lesbian women have more children than gay men, it would be unsurprising to see lesbian households switch more to a specialization of their household labor than gay men would, which would help explain why anti-discrimination laws have different effects on lesbian women and gay men.

To test the different responses by men and women in same-sex partnerships to anti-discrimination laws and the implications of Becker's household model, I implement an alternative difference-in-difference model. Instead of comparing those in same-sex partnerships to those in different-sex partnerships, I compare men and women in same-sex partnerships to examine their differential responses at the household level. Do women in same-sex partnerships change their household division of labor relative to men in same-sex partnerships following anti-discrimination laws? To answer this question, I collapse the data to the household level and examine households instead of individuals.

Specifically, I estimate this model:

$$y_{jct} = \alpha_0 Law_{ct} + \alpha_1 FemSSP_j + \alpha_2 Law_{ct} * FemSSP_j + \alpha_3 X_j + \mu_c + \delta_t + \epsilon_{jct}$$

The notation is the same as before with two main differences. My interaction term uses an indicator for if household j is a female same-sex partnership, denoted by $FemSSP_j$. y_{jct} is a variable for outcomes at the household level. α_2 is my coefficient of interest and will show how lesbian households differ in their response to anti-discrimination laws compared to gay households. The first outcome examined is an indicator for if the household is a one-earner family. I also examine the difference in absolute value between the two partners in terms of weekly hours worked. These labor supply measures inform how intrahousehold labor is divided up between partners along the extensive and intensive margin of labor supply. Finally, I examine

how the anti-discrimination laws may affect child rearing by examining if the households have any children and the number of children they have.

I present the results in Table II.10. Anti-discrimination laws do not differentially affect the likelihood of women in same-sex partnerships to become a one-earner household, but there is an effect on the intensive margin. There is a significant differential effect of anti-discrimination laws in the intrahousehold difference in hours worked for lesbian households. An increase in the difference of hours worked within the household could suggest a greater specialization in market production for one woman and a greater specialization in household production and working fewer hours for the other woman in the partnership. One potential reason for this increased specialization would be having more children and having a greater need for lesbian partnerships to specialize in household production. Lesbian households do see a significant increase in the likelihood of having any child and the number of children they have relative to gay households.

This evidence tied with Becker's theoretical work suggest that men and women in same-sex partnerships are differentially responding to these anti-discrimination laws. Lesbian households begin to have more children than gay households and change their labor supply and intrahousehold division of labor to accommodate their new children. It may appear in the main results that anti-discrimination laws hurt lesbian women because their labor supply and pay premium over straight women shrinks. However, their response in the labor market could be driven by a greater desire for children and subsequent changes in household labor allocation due to an increase in job security. The corresponding changes in the labor market could represent an increase in welfare for lesbian households if they value their new children and increased welfare from specializing greater than their lost hours worked and subsequent lost wages. Consistent with the theory and empirical findings, lesbian households could become more secure with one woman being the primary earner following anti-discrimination laws and the other woman working fewer hours to instead focus on household production and rearing children, which are more prevalent in lesbian households following the passage of anti-discrimination laws.

6.2 Mechanism

The results of this research indicate that sexual orientation anti-discrimination laws could significantly impact the labor market gaps for gay/bisexual men as well as the labor market premiums for lesbian/bisexual women. It is possible that these laws are binding and eliminate significant discrimination in the labor market. However, it is relatively easy for an employer to create a reason to fire an employee rather than firing someone specifically for being LGB. It is also difficult to accurately determine someone's sexual orientation by looking at them unlike race and sex. Although it may be easier to determine someone's sexual orientation conditional on them being in a same-sex relationship. Sexual orientation anti-discrimination laws may not be binding in a meaningful way that eliminates discrimination.

One mechanism that the laws could be affecting labor market outcomes is through increased positive sentiment toward LGB workers. I show in Figure II.6 that following state anti-discrimination laws there is a significant and persistent increase in the percentage of people in that state that support same-sex marriage. However, my regression results already condition on the change in the state-level support for same-sex marriage. They do not condition on the change in local-level sentiment toward LGB workers as that is unobservable. Given that there is a change in the sentiment at the state-level following a state law, it is likely that there may be some change in sentiment at the local level. I show the local laws matter significantly for the correct inference, so it seems plausible that local law changes could significantly change local sentiment and impact the labor supply and pay of LGB workers. Changing public sentiment in conjunction with greater protection for LGB workers seems more plausible as the mechanism than simply greater protection in the workplace.

6.3 Threats to External Validity

One limitation of this research is that I am unable to view all LGB workers. The analysis is specifically conducted on individuals in same-sex partnerships and different-sex partnerships. The data does not allow for the identification of single LGB workers, whose outcomes may be significantly

different than LGB workers in partnerships. Another pitfall to using partnerships to infer sexual orientation is that one could incorrectly infer someone's sexual orientation from a partnership. Bisexual people are a larger group than lesbian or gay people, and this specification could erroneously assign bisexual people to be "straight" through being in a different-sex partnership (Gates, 2011). If bisexual workers appear more similar to their lesbian and gay counterparts then the results of quantifying the labor supply and pay gap/premium would likely be biased toward zero and the change in those labor market gaps/premiums would also be biased toward zero.

Another potential concern in using partnerships to infer sexual orientation is that people in a same-sex partnership may be the group that is most at risk of discrimination. It seems likely that single LGB workers can more plausibly stay in the closet to their co-workers compared to their counterparts in a same-sex partnership. This analysis may be capturing the effect of anti-discrimination laws on the group that is most likely to be affected. These results may have a larger effect size than what one would find in examining the generalized LGB population.

These biases are with respect to the broader LGB population and external validity. Notably, these potential biases do not affect internal validity. Translating the effect of anti-discrimination laws for people in same-sex partnership to all other LGB workers is not immediately obvious. It is dependent on many factors that are unknowable in this analysis such as the wages and labor supply for single LGB workers and bisexual workers in different-sex partnerships and how discrimination affects LGB people differentially for those in same-sex partnerships.

7 Conclusion

This analysis is the first quasi-experimental research examining how both local and state anti-discrimination laws on sexual orientation affect the labor supply and pay gap/premium between LGB and straight workers. I construct a novel panel dataset on local anti-discrimination laws to properly capture the granular nature of anti-discrimination laws as well as incorporating Pew polling data on support for same-sex marriage for each state in each year to proxy and control for the unobservable sentiment to LGB workers, and I show that accounting for local laws is

necessary for proper inference. I find that anti-discrimination laws appear to decrease sexual orientation inequality in the labor market. Specifically, I find that anti-discrimination laws significantly reduce the gay labor force participation gap by 1.3 p.p. (18%), the employment gap by 1.4 p.p. (17%), and the wage gap by 2.8 p.p. (11%) and reduce the lesbian labor force participation premium by 1.3 p.p. (18%), the employment premium by 1 p.p. (14%), and the annual earnings premium by 12 p.p. (14%). I also show that one potential mechanism that anti-discrimination laws work through is by increasing positive sentiments toward LGB Americans as measured by the support of same-sex marriage. Finally, I explain the differential response to these laws between gay and lesbian households through Becker's household specialization model and support the explanation with empirical evidence.

In a majority of states, it is currently legal to fire someone solely based on their sexual orientation, and states have been less inclined to pass anti-discrimination laws recently. Arkansas and Tennessee even passed anti-anti-discrimination laws preventing cities from enacting anti-discrimination laws. Since 2009, more states have barred cities from protecting their residents than the number of states extending protection. My research gives a comprehensive look at a policy that Congress is considering in "The Equality Act" to extend federal protection to sexual orientation and gender identity. My research gives the most comprehensive look at policy and can meaningfully inform the policy discussion around anti-discrimination laws. Overall, this research suggests that states and the federal government should be giving greater protection to their LGB workers, and sexual orientation anti-discrimination laws can be effective at addressing sexual orientation inequalities in the labor market.

Tables

Table II.1. Timing of Sexual Orientation Anti-discrimination Laws

Year	State	City or County
2005 and Before	CA, CT, DC, HI, ME, MD, MA, MN, NV, NH, NJ, NM, NY, RI, VT, WI	Boulder, CO; Denver, CO; Fort Collins, CO; Gainesville, FL; Hialeah, FL; Hollywood, FL; Key West, FL; Miami, FL; Orlando, FL; Pembroke, FL; Saint Petersburg, FL; Tampa, FL; West Palm, FL; Atlanta, GA; Ames, IA; Cedar Rapids, IA; Davenport, IA; Des Moines, IA; Iowa City, IA; Campaigne, IL; Chicago, IL; Peoria, IL; Urbana, IL; Bloomington, IN; Fort Wayne, IN; Michigan City, IN; Terre Haute, IN; Lawrence, KS; Covington, KY; Lexington, KY; Louisville, KY; New Orleans, LA; Ann Arbor, MI; Detroit, MI; Grand Rapids, MI; Ypsilanti, MI; Columbia, MO; Kansas City, MO; Saint Louis, MO; Cleveland, OH; Columbus, OH; Toledo, OH; Eugene, OR; Portland, OR; Benton County, OR; Salem, OR; Allentown, PA; Erie, PA; Harrisburg, PA; Lancaster, PA; Philadelphia, PA; Pittsburgh; Scranton, PA; Austin, TX; Dallas, TX; Fort Worth, TX; Alexandria, VA; Arlington, VA; Seattle, WA; Spokane, WA; Tacoma, WA
2006	IL, WA	Dubuque, IA; Indianapolis, IN; Ferndale, MI; Lansing, MI; Cincinnati, OH; Easton, PA; West Chester, PA; Charleston, SC
2007	CO, IA, OR	Waterloo, IA; Coshocton, OH; Dayton, OH; Newark, OH; Charleston, WV
2008		Columbia, SC
2009	DE	Allegheny, PA; Reading, PA; Salt Lake City, UT
2010		Tallahassee, FL; Traverse City, MI; Missoula, MT; Lower Merion, PA; Grand County, UT; Summit County, UT
2011		Volusia County, FL; Evansville, IN; University City, MO; East Cleveland, OH; Bethlehem, PA; Conshohocken, PA; Haverford, PA; Ogden, UT
2012		St. Augustine, FL; Boise, ID; New Albany, IN; South Bend, IN; Flint, MI; Muskegon, MI; Maplewood, MO; Helena, MT; Omaha, NE; Canton, OH; Abington, PA; Cheltenham, PA; Morgantown, WV
2013		Phoenix, AZ; Pocatello, ID; Frankfort, KY; Shreveport, LA; Battle Creek, MI; Bristol, PA; Pittston, PA; San Antonio, TX; Charlottesville, VA; Huntington, WV
2014		Tempe, AZ; Adrian, MI; Macomb County, MI; Butte, MT
2015	UT	Anchorage, AK; Osceola County, FL; Anderson, IN; Clinton, IN; Hammond, IN; Muncie, IN
2016		Kokomo, IN; Manahattan, KS; St. Charles, MO; Jackson, MS; Lakewood, OH; Carlisle, PA; Dickson City, PA; Wilkes-Barre, PA; Martinsburg, WV; Wheeling, WV

Note: List of states, cities, and counties with sexual orientation anti-discrimination laws pulled from LGBTMap.org, an advocacy group, as well as through media reports and local FOIA requests. I only list city or county laws if there is no state law. Illinois passed their law in 2005 and enacted it in 2006.

Table II.2. Counties with Largest LGB Populations

County	State	Percent of SSPs
San Francisco County	CA	7.61
District of Columbia	DC	6.64
New York County	NY	5.50
Suffolk County	MA	4.38
Alexandria city	VA	3.95
St. Louis city	MO	3.90
Multnomah County	OR	3.82
DeKalb County	GA	3.37
Santa Fe County	NM	3.30
Baltimore city	MD	3.19

Note: Using the ACS person weights to recover the percentage of partnerships that are same-sex partnerships by county over 2005-2016.

Table II.3. Summary Statistics

Panel A: Men						
Variable	High School Grad or Lower			Some College or Higher		
	SSP n = 17,780	DSP n = 2,294,524		SSP n = 61,408	DSP n = 3,935,008	
Variable	Mean	Mean	Difference	Mean	Mean	Difference
In Labor Force	0.729	0.828	-0.099***	0.859	0.897	-0.037***
Employed	0.674	0.778	-0.104***	0.825	0.868	-0.043***
Annual Earnings	18280	23617	-5337***	46026	50923	-4897***
Hourly Wage	10.874	12.108	-1.234***	23.075	24.176	-1.102***
Age	46.206	47.219	-1.013***	45.683	46.768	-1.085***
Number of Children	0.545	1.190	-0.645***	0.266	1.122	-0.857***
Asian	0.028	0.028	0.000	0.042	0.060	-0.017***
Black	0.080	0.081	-0.001	0.041	0.058	-0.017***
Hispanic	0.111	0.114	-0.004*	0.063	0.047	0.016***
White	0.791	0.799	-0.008***	0.871	0.845	0.026***

Panel B: Women						
Variable	High School Grad or Lower			Some College or Higher		
	SSP n = 17,790	DSP n = 2,199,923		SSP n = 60,691	DSP n = 4,313,173	
Variable	Mean	Mean	difference	Mean	Mean	difference
In Labor Force	0.718	0.598	0.120***	0.861	0.746	0.115***
Employed	0.663	0.557	0.106	0.829***	0.718	0.111***
Annual Earnings	16273	10728	5545***	36757	24987	11771***
Hourly Wage	9.369	6.818	2.551***	18.975	14.573	4.401***
Age	45.736	47.972	-2.236***	45.016	45.261	-0.245***
Number of Children	0.744	1.086	-0.342***	0.514	1.106	-0.593***
Asian	0.024	0.043	-0.019	0.027***	0.064	-0.037***
Black	0.109	0.066	0.043	0.055***	0.057	-0.002***
Hispanic	0.093	0.116	-0.022***	0.053	0.051	0.002**
White	0.781	0.802	-0.021***	0.867	0.839	0.029***

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships. Summary statistics are presented by education level. T-tests were conducted to determine significant differences between those in same-sex partnerships and different-sex partnerships. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.4. Effect of Anti-Discrimination Laws: Extensive Margin of Labor Supply

Panel A: Men				
VARIABLES	(1) Labor Force	(2) Employed	(3) Labor Force	(4) Employed
Laws*SSP	0.013*** (0.005)	0.014** (0.005)	0.007 (0.005)	0.005 (0.006)
SSP	-0.074*** (0.003)	-0.079*** (0.004)	-0.068*** (0.003)	-0.073*** (0.004)
State Laws Only			X	X
Observations	6,287,441	6,287,441	6,287,441	6,287,441
R-squared	0.135	0.114	0.135	0.114
Panel B: Women				
VARIABLES	(1) Labor Force	(2) Employed	(3) Labor Force	(4) Employed
Laws*SSP	-0.013** (0.006)	-0.010* (0.006)	-0.016*** (0.006)	-0.013** (0.006)
SSP	0.081*** (0.004)	0.073*** (0.005)	0.080*** (0.004)	0.072*** (0.004)
State Laws Only			X	X
Observations	6,569,373	6,569,373	6,569,373	6,569,373
R-squared	0.091	0.088	0.091	0.088

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships with the regressions run separately by sex. All of the outcome variables are binary taking a value of 0 or 1. The first row of coefficients show the effect of anti-discrimination on the labor supply gap or premium, and the second row of coefficients give the labor supply gap or premium. Columns (3) and (4) present results when estimating only using state-wide anti-discrimination laws for comparison. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.5. Effect of Anti-Discrimination Laws: Intensive Margin of Labor Supply

Panel A: Men				
VARIABLES	(1) Weekly Hours	(2) Weeks Worked	(3) Weekly Hours	(4) Weeks Worked
Laws*SSP	0.273* (0.160)	0.089 (0.080)	0.180 (0.164)	-0.0744 (0.0820)
SSP	-2.196*** (0.114)	-0.575*** (0.056)	-2.106*** (0.0923)	-0.477*** (0.0480)
State Laws Only			X	X
Observations	5,244,258	5,244,258	5,244,258	5,244,258
R-squared	0.034	0.011	0.034	0.011
Panel B: Women				
VARIABLES	(1) Weekly Hours	(2) Weeks Worked	(3) Weekly Hours	(4) Weeks Worked
Laws*SSP	-0.305** (0.136)	-0.184** (0.084)	-0.213 (0.134)	-0.171** (0.0790)
SSP	2.792*** (0.105)	0.604*** (0.066)	2.706*** (0.0924)	0.574*** (0.0577)
State Laws Only			X	X
Observations	4,366,603	4,366,603	4,366,603	4,366,603
R-squared	0.038	0.015	0.038	0.015

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships with the regressions run separately by sex. Weeks worked in the ACS is a categorical variable giving a range of weeks worked. I take the median value given in the range to compute weeks worked. The first row of coefficients show the effect of anti-discrimination on the labor supply gap or premium, and the second row of coefficients give the labor supply gap or premium. Columns (3) and (4) present results when estimating only using state-wide anti-discrimination laws for comparison. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.6. Effect of Anti-Discrimination Laws: Wages and Earnings

Panel A: Men								
VARIABLES	(1) Hourly Wage	(2) Annual Earnings	(3) Hourly Wage	(4) Annual Earnings	(5) Hourly Wage	(6) Annual Earnings	(7) Hourly Wage	(8) Annual Earnings
Laws*SSP	0.028* (0.017)	0.059 (0.050)	0.011 (0.015)	-0.041 (0.035)	0.0139 (0.0184)	-0.00404 (0.0525)	0.00639 (0.0168)	-0.0581 (0.0373)
SSP	-0.244*** (0.012)	-0.757*** (0.038)	-0.082*** (0.009)	-0.114*** (0.023)	-0.233*** (0.0108)	-0.716*** (0.0354)	-0.0780*** (0.00745)	-0.112*** (0.0184)
Employed Only State Laws Only			X	X	X	X	X	X
Observations	6,287,441	6,287,441	5,244,258	5,244,258	5,244,258	6,287,441	5,244,258	6,287,441
R-squared	0.157	0.121	0.127	0.041	0.127	0.157	0.041	0.121
Panel B: Women								
VARIABLES	(1) Hourly Wage	(2) Annual Earnings	(3) Hourly Wage	(4) Annual Earnings	(5) Hourly Wage	(6) Annual Earnings	(7) Hourly Wage	(8) Annual Earnings
Laws*SSP	-0.019 (0.020)	-0.129** (0.066)	-0.024** (0.011)	-0.062** (0.029)	-0.0203 (0.0202)	-0.153** (0.0668)	-0.0144 (0.0112)	-0.0453 (0.0297)
SSP	0.220*** (0.014)	0.815*** (0.049)	0.053*** (0.007)	0.149*** (0.021)	0.218*** (0.0116)	0.809*** (0.0409)	0.0448*** (0.00635)	0.132*** (0.0180)
Employed Only State Laws Only			X	X	X	X	X	X
Observations	6,569,373	6,569,373	4,366,603	4,366,603	6,569,373	6,569,373	4,366,603	4,366,603
R-squared	0.136	0.106	0.123	0.036	0.136	0.106	0.123	0.036

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships with the regressions run separately by sex. Columns (3) and (4) are limited to those who are employed. All of the variables are in terms of 1999 dollars and received the $\log(x+1)$ transformation to have the interpretation of the coefficient be in terms of percent. The first row of coefficients show the effect of anti-discrimination laws on the pay gap or premium, and the second row of coefficients give the pay gap or premium. Columns (5) - (8) present results when estimating only using state-wide anti-discrimination laws for comparison. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.7. Anti-Discrimination Laws on Labor Supply with Occupation FEs

VARIABLES	Men		Women	
	(1) Labor Force	(2) Employed	(1) Labor Force	(2) Employed
Laws*SSP	0.009*** (0.003)	0.009** (0.004)	-0.009*** (0.003)	-0.009*** (0.004)
SSP	-0.037*** (0.002)	-0.045*** (0.003)	0.039*** (0.002)	0.036*** (0.003)
Observations	6,287,441	6,287,441	6,569,373	6,569,373
R-squared	0.372	0.461	0.517	0.461

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships with the regressions run separately by sex and include four-digit occupation fixed effects. All of the outcome variables are binary taking a value of 0 or 1. The first row of coefficients show the effect of anti-discrimination on the labor supply gap or premium, and the second row of coefficients give the labor supply gap or premium. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table II.8. Anti-Discrimination Laws on Pay with Occupation FEs

Panel A: Men				
VARIABLES	(1) Hourly Wage	(2) Annual Earnings	(3) Hourly Wage	(4) Annual Earnings
Laws*SSP	0.013 (0.013)	0.018 (0.035)	0.011 (0.013)	-0.028 (0.033)
SSP	-0.113*** (0.008)	-0.351*** (0.025)	-0.043*** (0.008)	-0.051** (0.022)
Employed only			X	X
Observations	6,287,441	6,287,441	5,244,258	5,244,258
R-squared	0.372	0.372	0.248	0.169
Panel B: Women				
VARIABLES	(1) Hourly Wage	(2) Annual Earnings	(3) Hourly Wage	(4) Annual Earnings
Laws*SSP	-0.015 (0.011)	-0.104*** (0.034)	-0.024*** (0.009)	-0.063** (0.025)
SSP	0.117*** (0.008)	0.437*** (0.028)	0.056*** (0.007)	0.172*** (0.018)
Employed only			X	X
Observations	6,569,373	6,569,373	4,366,603	4,366,603
R-squared	0.513	0.527	0.303	0.233

Note: Data comes from the 2005-2016 yearly ACS comparing people in same-sex partnerships to people in different-sex partnerships with the regressions run separately by sex and include four-digit occupation fixed effects. All of the variables are in terms of 1999 dollars and received the log(x+1) transformation to have the interpretation of the coefficient be in terms of percent. The first row of coefficients show the effect of anti-discrimination on the pay gap or premium, and the second row of coefficients give the pay gap or premium. Standard errors are clustered at the county level. *** p<0.01, ** p<0.05, * p<0.1

Table II.9. Sorting and Reporting

VARIABLES	(1) All SSP	(2) Male SSP	(3) Female SSP
Laws	0.000170 (0.000489)	0.000633 (0.000572)	-0.000304 (0.000573)
Observations	12,872,572	6,295,028	6,577,544
R-squared	0.068	0.072	0.065

Note: Data comes from the 2005-2016 yearly ACS seeing how the number of same-sex partnerships change in a county after the passage of an anti-discrimination law. The first column looks at both men and women with the next two columns separating the sexes. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

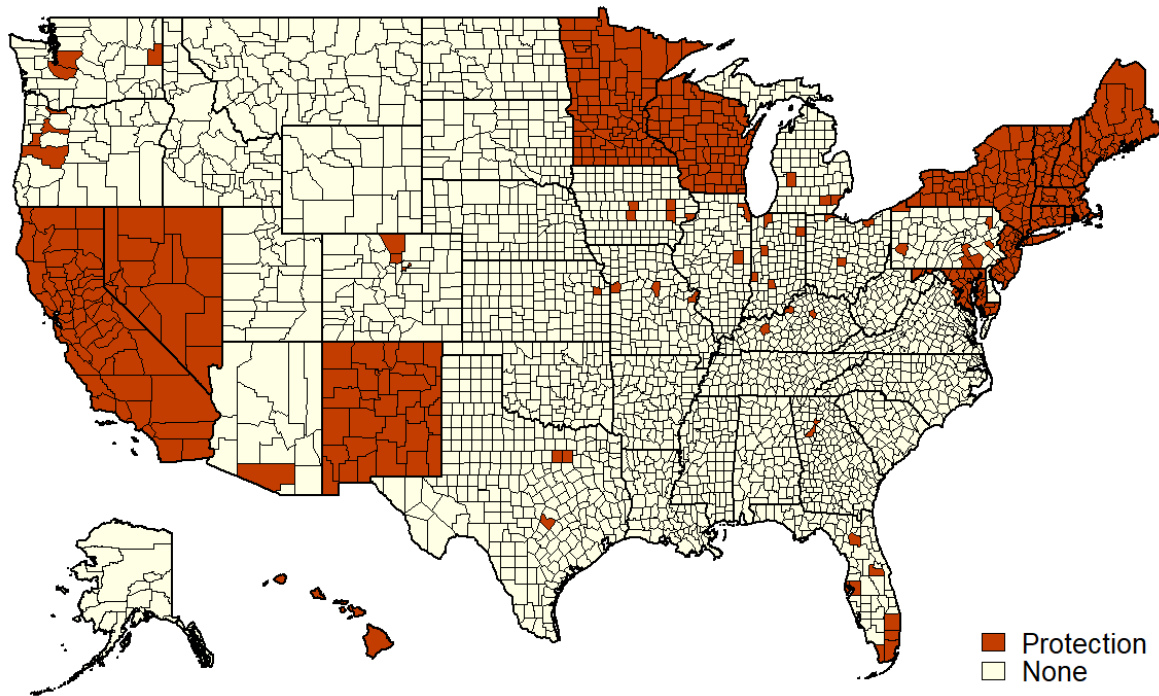
Table II.10. Differential Responses between Male and Female Same-Sex Partnerships

VARIABLES	(1) One Earner HH	(2) Diff in Hours Worked	(3) Any Children	(4) Number of Children
Laws*FemSSP	-0.00308 (0.00868)	0.947*** (0.364)	0.0344*** (0.00846)	0.0544*** (0.0195)
FemSSP	0.00445 (0.00717)	-0.729** (0.317)	0.123*** (0.00743)	0.193*** (0.0176)
Observations	73,181	73,181	73,181	73,181
R-squared	0.046	0.043	0.122	0.111

Note: Data comes from the 2005-2016 yearly ACS comparing women in same-sex partnerships to men in same-sex partnerships. The first column examines if the household has only one-earner. The second column tests the difference in absolute terms of hours worked between the two partners. The third and fourth columns examines how households differ with having children. The first row of coefficients show the differential effect of anti-discrimination between lesbian and gay households. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

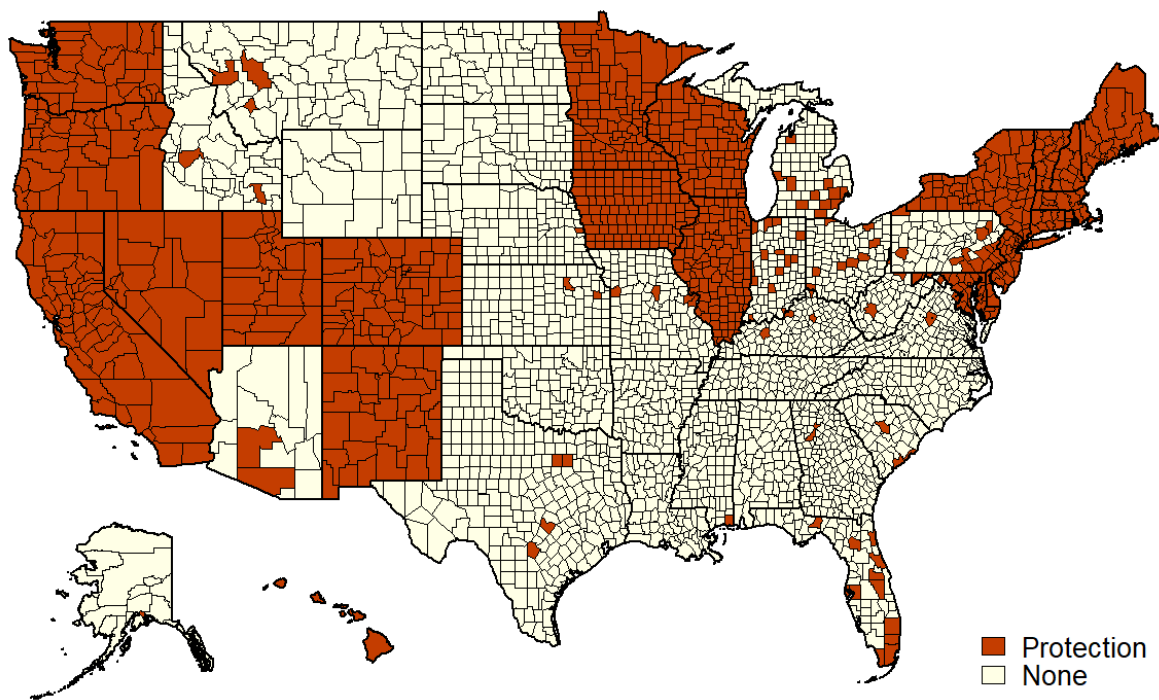
Figures

Figure II.1. Sexual Orientation Anti-Discrimination Laws: 2005



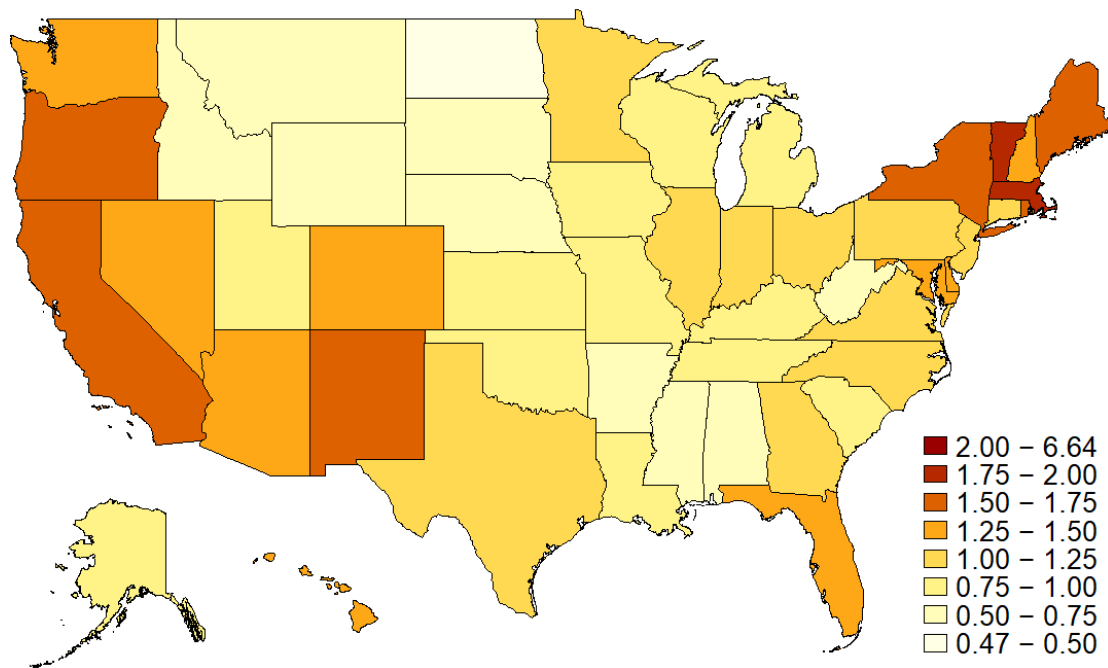
Note: State and local sexual orientation employment anti-discrimination laws in 2005. Data on laws obtained from LGBTMap.org and author's own investigation using media reports and FOIA requests.

Figure II.2. Sexual Orientation Anti-Discrimination Laws: 2016



Note: State and local sexual orientation employment anti-discrimination laws in 2016. Data on laws obtained from LGBTMap.org and author's own investigation using media reports and FOIA requests.

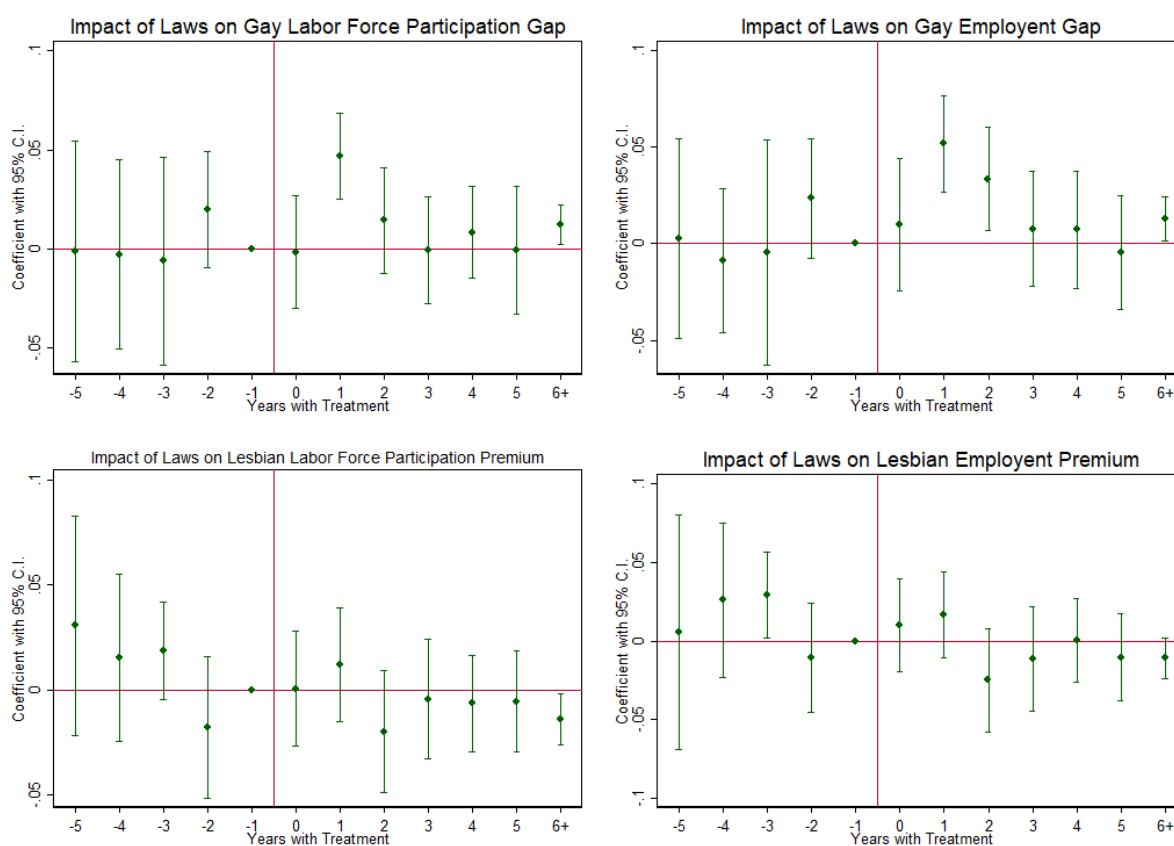
Figure II.3. Percentage of Same-Sex Partnerships: 2005-2016



Source: American Community Survey

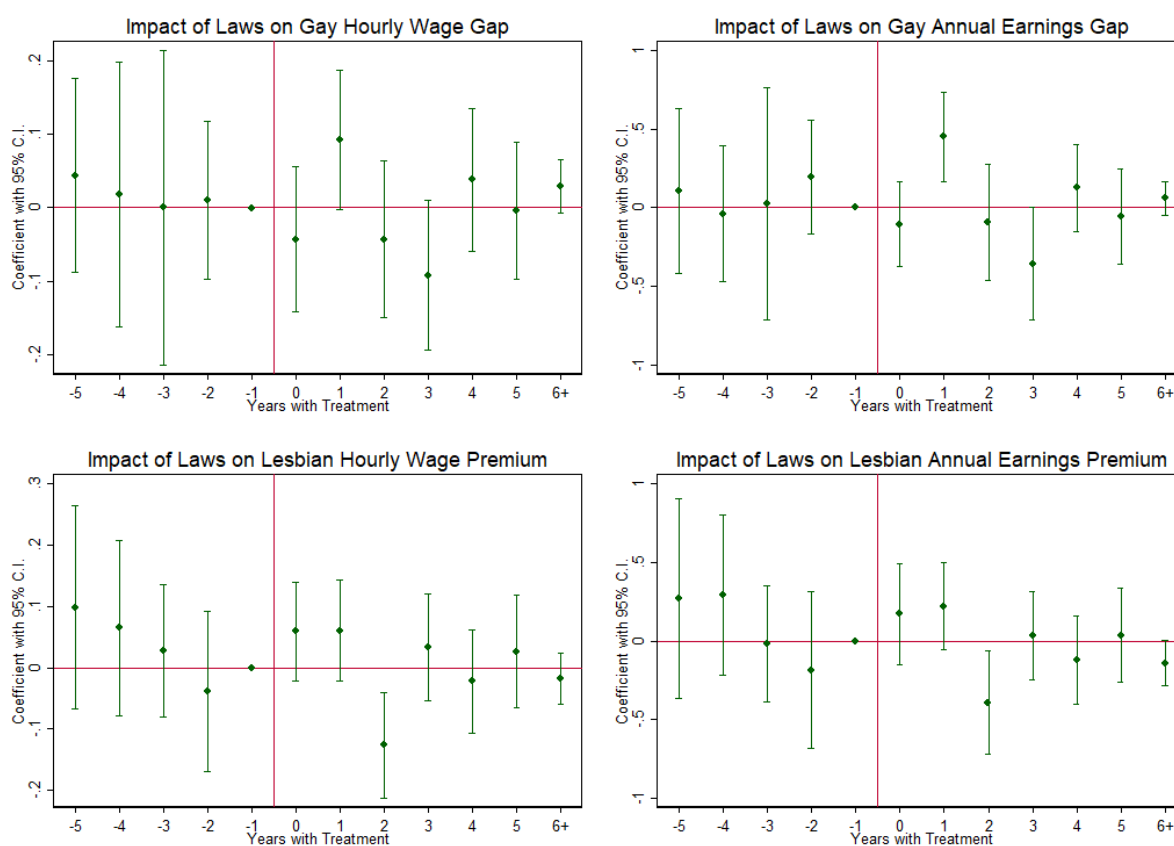
Note: Percentage of partnerships that are same-sex for each state and DC over 2005-2016 using the American Community Survey. Author's calculations

Figure II.4. Impact on Labor Supply Differentials



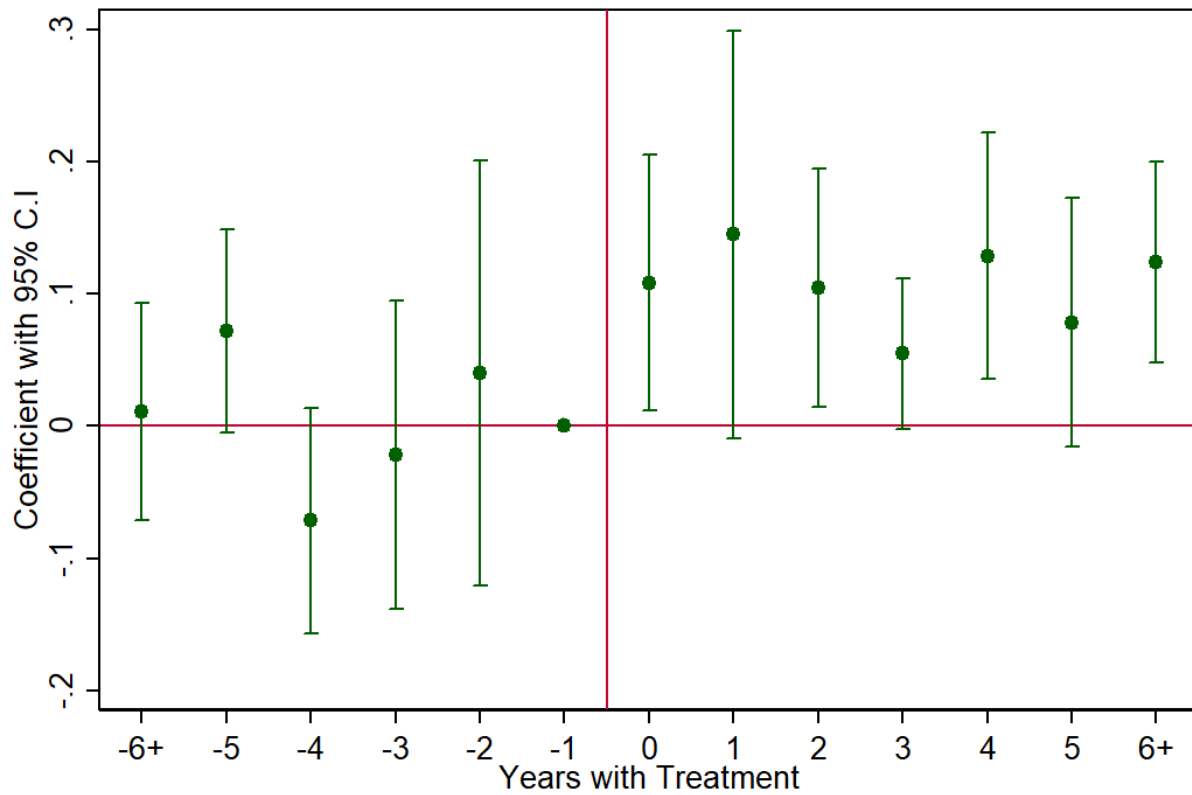
Note: Event study plot on the difference in labor supply between people in same-sex partnerships and different-sex partnerships broken down by sex following the county-level and state-level anti-discrimination laws. Standard errors are clustered at the county level.

Figure II.5. Impact of Wage Differentials



Note: Event study plot on the difference in pay between people in same-sex partnerships and different-sex partnerships broken down by sex following the county-level and state-level anti-discrimination laws. Standard errors are clustered at the county level.

Figure II.6. Impact of Laws on Support for Same-Sex Marriage



Note: Event study plot showing how support for same-sex marriage changes following state-level anti-discrimination laws. Polling data comes from Pew Polling and encompasses 2005-2016 for every state excepting Hawaii and Alaska, which are missing for 2005-2008. Standard errors are clustered at state level.

Chapter III

PrEP and Moral Hazard¹

1 Introduction

HIV and AIDS have devastated lives by cutting them short, hitting the gay community hardest. At the start of AIDS epidemic in the early 1980s, an HIV or AIDS infection effectively sentenced one to death, but as investment in medical interventions and treatments improved, so did health outcomes. One potential concern with new medical treatments is the potential of unintended consequences of people adjusting their behavior to new medications, attenuating the effect of an intervention. Researchers have shown significant moral hazard responses to HIV medical breakthrough, wherein people adjust their behavior and engage in riskier sex practices if the consequence of that behavior becomes less dangerous (Lakdawalla, Sood and Goldman (2006); Chan, Hamilton and Papageorge (2015)).

We examine the effect of a new HIV treatment, Pre-exposure Prophylaxis (PrEP), a drug that effectively prevents HIV infections, on aggregate STDs and HIV infections. Approved nationally in 2012, PrEP has the potential to save lives but also carries a potential risk of moral hazard with it. PrEP has been clinically shown to be effective at preventing new HIV infections in the medical literature, and it has steadily gained use and popularity among men who have sex with men (MSM). Users of PrEP may rationally adjust their sexual behavior and may have sex with more partners and may stop using condoms given the drastically decreased likelihood of contracting HIV. PrEP may reduce the number of HIV infections, but it may also give a trade-off of increased

¹This work is joint with Nir Eilam, and I am the primary author on this chapter.

STDs. However, PrEP could also possibly have no effect on HIV infections if all PrEP users perfectly substituted condoms with PrEP, underlying the importance of examining the effect of PrEP on STDs and HIV.

We are the first to examine the relationship between the roll out of PrEP and aggregate STD and HIV rates. We contribute to the medical literature that has examined PrEP, but importantly, they have not viewed the drug in the context of aggregate STD levels, focusing on the people that actively take PrEP. This approach could underestimate the effect that PrEP has on aggregate level since STDs can spread to people who are not taking PrEP. We also add to the literature on moral hazard as well as contributing to an open question on increasing STD rates.

This research sheds important light for relevant policy discussions as well. First, cases of chlamydia, gonorrhea, and syphilis were at an all time high in 2018, underlying the importance of understanding the factors causing these STDs to rise in recent years. Next, President Donald Trump announced in the 2019 State of the Union a plan to eliminate HIV infections within 10 years with expanding PrEP access as an integral part of the plan. Finally, a planned rollout of a generic PrEP alternative in 2020 by Gilead combined with the plan to eliminate HIV infections highlights the role this research plays in understanding the tradeoffs of expanding PrEP access and increasing STD rates.

We obtained yearly data on state level PrEP usage per 100,000 for 2012 to 2017 from AIDSvu, an organization created by Emory University's Rollins School of Public Health and Gilead Sciences Inc., the company that manufactures PrEP. We paired that data on STD rates per 100,000 from the Center for Disease Control (CDC) from 2008 to 2017 for chlamydia, gonorrhea, syphilis, and HIV infections. We focus on male PrEP usage and STD rates.

A basic difference-in-difference analysis leveraging differential adoption of PrEP after 2011 across states show significant associations between male PrEP rate and male STDs. However, there could be potential issues of endogeneity where states with increasing STD or HIV rates are most likely to adopt PrEP. We cannot estimate the effect of PrEP in a typical difference-in-differences framework which leverage differential start times because it was introduced nationally in 2012. To ameliorate these concerns about differential pre-trends in STDs, we exploit a

pre-treatment measure of the intensity of PrEP exposure in a difference-in-differences framework. Specifically, we follow analyses by Bleakley (2007) and Alpert, Powell and Pacula (2018), which examine treatments without staggered rollouts using pre-treatment variation. Bleakley (2007) uses pre-treatment measures of hookworm exposure to examine the effect of hookworm eradication in the American South. Alpert, Powell and Pacula (2018) examines a nation-wide reformulation of OxyContin making abuse of the pills more difficult. They use the pre-treatment abuse of opioids before the national change to show those states had the largest increase in heroin abuse following the OxyContin reformulation.

Following a similar strategy, we use pre-treatment variation in the gay male population, which we show are the areas that use PrEP the most. We show that prior to the introduction of PrEP in 2012, the rates of male STDs are parallel in states with high and low gay populations and subsequently high and low PrEP adoption rates, and after 2012, STD rates diverge with male STDs increasing in higher gay population states. However, we find that HIV rates are consistently declining before the introduction of PrEP in states with a large gay population with no break when PrEP is introduced, making it difficult to estimate the casual effect of PrEP on aggregate HIV levels. Given that HIV infections are declining faster in states with a larger gay population, our estimates on HIV infections will likely be downwardly biased, overstating the effect of PrEP on HIV infections.

Specifically, we find that a 1 additional male PrEP user results in 0.575 additional cases of male chlamydia, 0.61 additional cases of male gonorrhea, 0.032 additional cases of male syphilis, and 0.095 fewer cases of male HIV². We estimate these results using state-specific linear time trends to best control for differential trends in STDs.

Finally, we conduct several back of the envelope calculations. First, we conduct a counterfactual analysis to estimate how much higher male STDs are in 2017 due to PrEP. We find that in 2017 male chlamydia, male gonorrhea, and male syphilis cases are 9%, 18%, and 11% higher than they would have been in the absence of PrEP. While the counterfactual suggests a large amount of the increase in male STDs can be explained by PrEP, the CDC reports that gay men make up

²Again, the HIV estimate is likely overstating the effect of PrEP on HIV

a plurality of all gonorrhea and syphilis cases compared to straight men and all women. Given how much gay men contribute to male gonorrhea and syphilis rates, the counterfactual analysis is plausible.

Next, we conduct a naïve cost-benefit analysis. However, given the relative trends in HIV diagnoses before the introduction of PrEP, we will overestimate the benefits of PrEP, which is the effect of PrEP on HIV. We take the lifetime cost in 2010 dollars of treating different STDs from the medical literature taken from Owusu-Edusei Jr et al. (2013) and calculate the costs and benefits of PrEP. If one were to believe the biased estimate of PrEP on HIV, then PrEP was overwhelmingly welfare improving with about \$2.7 billion in benefits in 2017. However, the benefits of PrEP are likely overstated given the difficulty of identifying the effect of PrEP on HIV. Given that we are not confident in the calculation of benefits, we estimate a break-even analysis instead. We find that in 2017, PrEP increased male chlamydia, gonorrhea, and syphilis rates to a level that cost approximately \$8.3 million. Given the lifetime cost of treating HIV is about \$300,000, the 94,196 additional male PrEP in 2017 would need to prevent only 28 cases of male HIV, a prevention rate of 0.03%, for PrEP to be welfare improving and to offset the costs of the additional STD treatment. We estimate the effect of PrEP on HIV to be over 300 times the needed prevention rate, but again, our estimate is likely biased to be too large.

We present the first analysis showing that PrEP has a significant, causal effect on aggregate STDs, costing millions of dollars a year for treatment. Specifically, we show that one additional male PrEP user increases male chlamydia infections by 0.575 cases, gonorrhea by 0.61 cases, and syphilis by 0.032 cases. In total, our results suggest that one additional male PrEP users adds about 1.22 additional male STDs. We cannot confidently identify the effect of PrEP of HIV given the relative trends in HIV infections before the introduction of PrEP. Given the difficulty in estimating the benefits, we show that given the costs of treating different STDs and HIV, that only 28 cases of HIV need to be prevented in 2017 for PrEP to be welfare improving. Given how few cases of HIV are needed to be prevented to be net-positive and how many male PrEP users there are, it is highly likely that PrEP's introduction is welfare improving.

2 Literature Review

The medical literature has studied PrEP usage for several years, examining the effectiveness of PrEP along with examining potential moral hazard factors of the HIV-prevention drug. We are the first in the social science literature to examine PrEP and the potential moral hazard consequences of increased risky behavior and corresponding STDs.

There are several reasons why one may want to examine this question through the lens of social science. First, the medical literature has not examined the effect of PrEP on aggregate STD levels. They have mostly been confined to looking at the people who are taking PrEP, and it seems plausible that there are potential spillovers of STDs to the non-PrEP community. Second, there is the potential of the Hawthorne effect where enrolling PrEP takers into a study may impact their behavior (Adair, 1984). Enrolling someone into a study has been shown to alter behavior and the medical literature on PrEP is not immune to those concerns. By using observational data on PrEP and STDs, we can circumvent this concern. Finally, the medical literature has found varying effects on how PrEP affects sexual behavior.

The medical literature varies in their approach and findings in examining the effect of PrEP on increased risky behavior. Even the meta-analyses differ in their findings of the effect of PrEP on STDs. Traeger et al. (2018) examines 8 studies on STD incidence and 13 studies on condom usage, and they find significant increases in rectal chlamydia and any STD diagnosis with a stronger association in the later studies. They also find that condomless sex increases in most studies. However, Freeborn and Portillo (2018) conducts a different meta-analysis and found no conclusive evidence on increased STDs.

Some studies focus mostly on surveying MSM before and after administering PrEP while taking regular HIV and STD tests. Volk et al. (2015) used administrative data and surveys of MSM in San Francisco and found that after a year, 50% had any STD, 33% had a rectal STD with 33% having chlamydia, 23% having gonorrhea, and 6% having syphilis. Volk et al. (2015) also found condom use decreased for 41% and number of sexual partners increased for 11%. Marcus et al. (2016) found similar results with total of 771 STD diagnoses for 972 PrEP users with many people having multiple STDs. They found that after a year, 42% had any STD, 27% had a rectal STD, 26%

had chlamydia, and 23% had gonorrhea. There were significant increases over the baseline for chlamydia and gonorrhea diagnoses.

One encouraging aspect of the findings of the literature and our findings is that we see a larger effect in the change in aggregate level of STDs than what these medical studies find as we would expect larger growth in aggregate STDs given the infectious nature of STDs. Marcus et al. (2016) found that one PrEP user resulted in 0.79 additional STDs, which is about slightly more than half of what we find in the aggregate STDs, which is intuitive given that one person testing positive means that at least two people have the disease since they contracted it from someone else.

However, there are some studies that find no change in relative STD rates. Liu et al. (2016) surveyed MSMs before and after PrEP and found that the proportion of those having condomless receptive sex stayed constant at 65% with an insignificant increase in STDs. McCormack et al. (2016) conducted an RCT in the UK. They randomized their sample into people who received PrEP immediately and those who would receive it after 1 year. However, they cut the study short as some participants in deferred PrEP group began to contract HIV, and the authors felt ethically obliged to give people in the control group PrEP. Those who immediately received PrEP reported more condomless anal receptive sex and had higher incidences of STDs, but the significance of the increased STDs went away after controlling for additional STD screenings.

Even though the medical literature may have different findings on whether STDs increased or decreased as a result of PrEP, they consistently find that the STD incidence at the baseline was already high before administering PrEP (Volk et al. (2015); Liu et al. (2016); McCormack et al. (2016); Hosek et al. (2017)). Hosek et al. (2017) surveyed young MSM on PrEP and found that 80% reported condomless sex and 58% reported condomless receptive sex. The MSM population that would be taking PrEP already engages in relatively risky sexual behavior that by some studies becomes even riskier following PrEP.

We also contribute to the on-going epidemiology literature about the recent rise in STDs in the United States. The CDC released figures showing that cases of chlamydia, gonorrhea, and

sypilis were at all-time high in 2018³. One potential factor that they cite is “decreased condom use among vulnerable groups, including young people and gay and bisexual men.” The CDC with Health and Human Services (HHS) are prioritizing the rise of STDs and are developing a federal action plan to lower STDs, which will be released in 2020. Our research examines the role of PrEP in the recent rise of STDs, underlying the policy importance of this research. There are also additional policy implications in this research with respect to eliminating HIV and AIDS in the United States. In the 2019 State of the Union, President Donald Trump announced a plan to reduce HIV infections by 90% in the United States by 2030. HHS set up “Ready, Set, PrEP,” a national program to increase PrEP usage by providing for free to people who qualify⁴. PrEP usage will increase as a result of these initiatives as well as approval for a generic version of PrEP, which Gilead announced would be released in September 2020 in a SEC filing⁵. The combination of generic PrEP, the United States’ goal of eliminating new HIV infection, as well as the rapid increase of STDs highlights the importance of this research in developing a holistic policy to tackle HIV and STDs together.

This research also contributes to the robust economics literature on moral hazard. Economics has a long literature on moral hazard and unintended consequences with seminal work by Peltzman (1975), which suggested that innovations in driving safety would be muted through increased risky behavior. Cohen and Einav (2003) found small changes in behavior from seat belts relative to what Peltzman hypothesized. However, Cohen and Dehejia (2004) found that automobile insurance incentivized riskier driving through moral hazard and caused an increase in traffic fatalities.

Moral hazard is not limited to instances of insurance though. There is a growing literature on medical breakthroughs having unintended consequences. In particular, broadening naloxone access, a drug preventing opioid overdoses, led to more opioid related emergency room visits and opioid related crime with no decrease in opioid deaths (Doleac and Mukherjee, 2018). The same study showed that broadening naloxone access also increased risky behavior as seen by increased use of fentanyl, a more potent opioid than heroin (Doleac and Mukherjee, 2018).

³<https://www.cdc.gov/nchhstp/newsroom/2019/2018-STD-surveillance-report-press-release.html>

⁴<https://www.hiv.gov/federal-response/ending-the-hiv-epidemic/overview>

⁵The announcement can be found on page 35 of this document <http://investors.gilead.com/static-files/0ff8d741-b9eb-4162-bcb4-f16094d37254>

In a context closer to our own, Lakdawalla, Sood and Goldman (2006) consider the moral hazard effects of HIV treatment breakthroughs on risky sexual behavior. They find that treating HIV-positive individuals more than doubles their number of sexual partners and contributed to a large increase in HIV incidence during the same period. Chan, Hamilton and Papageorge (2015) provide a dynamic model of this behavioral response to the availability of life-saving HIV treatment. They show that both HIV-negative and HIV-positive men increase their risky sexual behavior when the cost of contracting HIV falls.

We contribute to the literature in multiple ways. First, we are the first non-medical study examining the moral hazard impacts of PrEP. Second, we are the first to document the effect of PrEP on aggregate STD levels, which is critically important given that diseases can have spillovers and infect those that are not on PrEP. Third, while the medical literature has documented that PrEP is effective at preventing HIV infections, it is unclear if that finding translates to the aggregate level of HIV diagnoses as people may be substituting away from condom use in replace of PrEP. We also contribute more broadly to the economics literature on moral hazard and unintended consequences and to the epidemiology literature on increasing STD rates. Importantly, our research is also incredibly timely and relevant for the United States federal government's plan of jointly reducing HIV infections and STD rates.

3 Data

We use data on four of the most common STDs: chlamydia, gonorrhea, syphilis, and HIV, which comes from the CDC National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention (NCHHSTP) database⁶ It contains information on the number of cases of each STD at the state and year level as well as the population of each respective group. Using the number of cases and the population we construct our outcome variable – the STD rate per 100,000 population. Data is available for all of the 50 states and the District of Columbia. We break the STD data by state

⁶CDC reports 3 types of Syphilis - primary and secondary, early latent and congenital. We use primary and secondary as that is what is a typical syphilis case with symptoms appears as. Latent syphilis has no symptoms, so one would be unlikely to seek treatment for it, and congenital syphilis is when a mother passes syphilis to a newborn child, which is not a relevant mode of transmission for this study.

by year for the years 2008-2017. We focus on 2008-2017 as our main sample for consistency since the NCHHSTP does not provide HIV records before 2008. We expand our analysis to 2000, the earliest year of reporting for chlamydia, gonorrhea, and syphilis rates in our results section as well.

For our main sample, we use persons aged 13 and up. During our sample years, PrEP was only approved for persons aged 18 and up, although it was occasionally prescribed off-label for younger persons (Highleyman, 2018). Magnuson et al. (2018) reports that only 2,324 males aged 12-19 started using PrEP over the years 2012-2017 across the United States, compared to 16,739 males aged 20-24.

Our PrEP usage data comes from AIDSVu, an online source for HIV related data. AIDSVu reports the number of PrEP users and rate per 100,000 at the state, year, and sex level as well as separately by the state, year, and age level. The AIDSVu PrEP data was obtained by AIDSVu by researchers at the Rollins School of Public Health at Emory University in conjunction with Gilead Sciences, Inc., the manufacturer of PrEP. The data is based on Symphony Health patient-level prescription data from a sample of pharmacies, hospitals, outpatient facilities, and physician practices across the United States. It encompasses all prescription payment types, including Medicare Part D and Medicaid. Since the prescriptions were for Emtricitabine/Tenofovir Disoproxil Fumarate, which is also used for other HIV treatment, Gilead used a stringent algorithm to separate the prescriptions that were PrEP. Prescriptions that could not be attributed to a specific indication were removed, although a certain share of those were PrEP. In addition, prescriptions from certain closed healthcare systems that did not share data with Symphony Health were not included. Therefore, the PrEP use data slightly underestimates the number of PrEP users. A minimum duration of 30 days was required for an individual to be considered a PrEP user, and to be considered a user in a given year, at least one day of that 30-day minimum period was required to fall within that calendar year.

Given that this drug is targeted toward gay men, we want to examine how this treatment differentially affects states with a higher gay male population. Unfortunately, there is little data on the distribution of the LGBT population, so we use data on same-sex partnerships from the

2000 Census. The 2000 Census asks about household composition, which we use to infer sexual orientation. We proxy for the gay male population in each state with data on male same sex partnerships from the 2000 Census. Measuring the gay male population using the Census would likely underestimate the amount of gay men since it is only capturing gay men willing to say they are in a committed relationship on the Census. However, it should give an approximation of the relative ranks of states with respect to the size of their gay male population, i.e. California has a higher percentage of same-sex partnerships than Wyoming even if both are underestimated. We calculate the share of male same-sex partnerships by dividing the number of male same-sex partnerships and dividing that by the total number of partnerships in a state.

Additional demographic and economic covariates at the state and year level are derived from the yearly American Community Surveys as well as the University of Kentucky Center for Poverty Research National Welfare Data for 2008-2016. These covariates include the racial makeup of the state, the natural logarithm of state GDP, the Supplemental Nutritional Assistance Program (SNAP) recipients, the unemployment rate, the poverty rate, and the state minimum wage.

3.1 Descriptive Statistics

Table III.1 provides summary statistics for states throughout our time period as well as split by those states that had the lowest and highest PrEP expansion. We split the states in the quartiles based on their PrEP expansion. “Low PrEP” states are in the lowest quartile of PrEP take-up, whereas states labeled “High PrEP” states are in the highest quartile of PrEP take-up. We present the statistics for the whole sample as well as split between pre-PrEP in 2008-2011 and post-PrEP in 2012-2017.

We detail the male rate for different STDs for our overall sample and between low and high PrEP states before and after the adoption of PrEP. Before the introduction of PrEP, high and low take-up states had similar male chlamydia and gonorrhea rates with 298 cases per 100,000 of male chlamydia in high PrEP states compared to 293 cases per 100,000 for in low PrEP states and 110 cases of male gonorrhea in high PrEP states versus 122 cases per 100,000 low PrEP take-up states. High PrEP take-up states had much higher rates of male syphilis and HIV relative to the low-PrEP

states though. We also show that our treatment in male PrEP rate, defined as users per 100,000, has a large difference between high and low PrEP take-up states. We also exploit the variation in the percentage of male same-sex partnerships in the 2000 Census, which is defined between 0 and 100 for interpretation of regression coefficients. High PrEP take-up states had higher percentage of male same-sex partnerships at 0.69% compared to 0.39% for low PrEP take-up states. We also present our state by year level control variables. High PrEP take-up states were also richer and had more Black and Hispanic people relative to the low PrEP take-up states.

First, Figure III.1 illustrates the overall number of PrEP users per 100,000 population separately for male and female. Since its introduction in 2012, the number of male users grew rapidly, which makes sense given the appeal toward gay men for HIV prevention. By 2017, there were 94,146 male users and 6,045 female users which corresponds to a PrEP rate of 70.6 for male and 4.3 for female. While the female PrEP rate remained rather constant over time, the male PrEP rate grew 1,800% since its introduction in 2012. Given heavy marketing brought by the warm endorsement of various public health organizations, the introduction of generic PrEP, and the Trump administration plan to eradicate HIV infections by 2030, the fast adoption of PrEP is unlikely to halt⁷. Our analysis will focus on the effect of male PrEP usage on male STDs given how much more prevalent this drug is for men.

Next, we show that in some states, users barely adapted PrEP while other states saw large increase in PrEP users. In Figure III.2, we show the male PrEP rate across states in 2017, illustrating this spatial variation. States in the West Coast and Northeast as well as Florida, Georgia, and Illinois were largest adopters of PrEP. Unsurprisingly, these states have a larger share of the gay population, which we show in Figure III.3, displaying the percentage of the partnerships in a state that are same-sex and male in the 2000 Census. Given that this drug is marketed toward and taken by gay men, the distribution of the gay men before the introduction of PrEP should be indicative of the states that would be more likely to adopt PrEP.

Next, we plot the evolution of male STD rates for states with different PrEP take-up. We divide states based on their quartiles of PrEP take-up in 2017. We then plot the evolution of the average

⁷<https://www.hiv.gov/federal-response/ending-the-hiv-epidemic/overview>

male STD rates for each quartile of states for the four STDs separately. The first quartile is the quartile of states that had the highest PrEP take-up, whereas the fourth quartile is the quartile of states that experienced the lowest PrEP take-up. We present the trends in STDs rate in Figure III.4.

Male STD rates, with the exception of HIV, were trending upwards in all states during the past few years, at a faster pace than previous years, to reach record highs each year. Between 2012 to 2017, male chlamydia, gonorrhea, and syphilis rates have increased on average by approximately 36%, 106% and 91%, respectively, compelling the CDC to call for urgent action⁸.

Figure III.4 shows that the states that experienced the fastest increases in recent years in male chlamydia and gonorrhea rates were the states in the highest quartile of PrEP take-up. The chlamydia and gonorrhea rates seem to be evolving in parallel for all of the states based on PrEP take-up before PrEP is introduced, suggesting these states may be good counterfactuals of each other. After PrEP is introduced, states with the largest increase in PrEP take up see the largest increases in STDs. In 2008, male chlamydia rates in states with high PrEP take-up were similar to states with lower PrEP take-up, but by 2017, they were approximately 20% higher with a similar finding for gonorrhea. Syphilis rates began increasing at a faster pace in states with high PrEP take-up 2 years prior to the introduction of PrEP, and increased at a faster rate after.

With respect to male HIV rates, Figure III.4 shows that rates were declining throughout the period for states in the top 3 quartiles of PrEP take-up. Moreover, the higher the PrEP take-up, the faster the decline in rates. This decline could be a result of efforts of numerous public health organizations groups that began in the early 2000s to combat HIV. These efforts were concentrated in states with high male HIV rates, which were the states that had large gay populations, which are also the states that had higher PrEP take-up once it was introduced. These efforts to reduce HIV could have been coupled with efforts to combat other public health concerns such as STDs. This simultaneous reduction does not seem the case, since although the reduction in HIV occurred throughout the period, most male STD rates were quite flat at the beginning of the period. The fact that male HIV rates were trending differently prior to the introduction of PrEP across states

⁸<https://www.cdc.gov/media/releases/2017/p0926-std-prevention.html>

with different PrEP take-up, provides a challenge for the identification of the effect of PrEP on male HIV rates as states with lower PrEP take-up may not provide an accurate counterfactual for male HIV rates in states with higher PrEP take-up. Estimating the effect on HIV would likely bias any negative effect downward from the downward trends of states with high PrEP take-up that existed before the introduction of PrEP. We mainly focus on the effects of PrEP on other STDs because of the difficulty in estimating the effect on HIV.

4 Identification Strategy

In order to best estimate the effect of PrEP on STDs we employ different specifications. First, we employ a simple differences-in-differences framework to see how the change in PrEP usage affects changes in STD. Our difference-in-difference estimate is limited to men in this equation and is defined as:

$$STD_{st} = \beta PrEP_{st} + \gamma X_{st} + \mu_s + \tau_t + \epsilon_{st} \quad (\text{III.1})$$

Where STD_{st} is the rate of STDs defined as cases per 100,000 in state s in year t . We examine chlamydia, gonorrhea, syphilis, and HIV separately. $PrEP_{st}$ is the PrEP rate per 100,000. We include a set of demographic and economic controls in X_{st} , which include racial demographics of each state, state GDP, and the number of SNAP recipients, the poverty rate, and unemployment rate. Finally, we include state- and year-fixed effects in μ_s and τ_t , respectively.

Our coefficient of interest is β , which gives the effect of one additional male PrEP user on male STD cases because $PrEP_{st}$ and STD_{st} are both defined as rates per 100,000 people. As we include state and year fixed effects, the effect is identified from the changes in PrEP rates within the same state over time and relative to the corresponding changes in other states. We run the regression separately for each STD. The regressions are population weighted and the errors are clustered at the state level.

The identifying assumptions is that male STDs in states with different PrEP rates would have evolved in parallel in the absence of PrEP. While this assumption is inherently untestable, we

can examine what how the trends in STDs evolved before and after the treatment, which we will discuss further in the next section.

4.1 Intensity of Pre-Treatment Variation

One of the difficulties in estimating the effect of a national rollout of a treatment is that there is little temporal variation to examine how trends differed in a traditional event study. One common strategy to get around this lack of differential treatment starts is to use pre-treatment variation as an intensity of treatment. Pre-treatment variation has been used to analyze the effect of hookworm eradication, the effect of making opioids harder to abuse, and the effect of the Affordable Care Act's free contraception mandate (Bleakley (2007); Alpert, Powell and Pacula (2018); Willage (2020)). Given that PrEP is targeted toward gay men given their unique risk of contracting HIV, we exploit the pre-treatment variation in the gay population as the states with a larger gay population would likely be the states that had the largest increase in PrEP utilization. We use the household information in the 2000 Census to infer sexual orientation based on if someone is in a same-sex partnership, which is common in the literature on LGBT economics (Klawitter and Flatt (1998); Black et al. (2003); Gates (2009)). Specifically, we estimate the below equation:

$$STD_{st} = \sum_{\substack{t=2008 \\ t \neq 2011}}^{2017} \beta_t \mathbb{1}(Year = t) * \%MaleSSP_s + \gamma X_{st} + \mu_s + \tau_t + \epsilon_{st} \quad (III.2)$$

Where the notation is the same as the previous equation in this section with some additions. $\%MaleSSP_s$ gives the percent of partnerships that are male same-sex partnerships in state s from the 2000 Census⁹. $\%MaleSSP_s$ is defined between 0 to 100 for better interpretability of regression coefficients. We have indicators for each year that are interacted with $\%MaleSSP_s$ with the exception of 2011, which is used as the comparison year because it is the year before PrEP was introduced. The interpretation on β_t is how many additional cases per 100,000 of an STD occur in year t with a 1 p.p. increase in the percent of male same-sex partnerships. For

⁹The Census asks for people in a household to list their relationship to the head of the household. Partnerships are defined as households with two adults that are either married or in a partnership.

context, the difference in the male same-sex partnerships between North Dakota and New York is about 0.5 p.p., so $\beta_t/2$ would give the predicted effect between North Dakota and New York. We run the regression separately for each STD and for male and female. The regressions are population weighted and the errors are clustered at the state level. For the sample going back to 2000 for chlamydia, gonorrhea, and syphilis rates, we estimate the same equation with additional year indicators for each year from 2000 to 2017.

We can also estimate the effect of the $\%MaleSSP_s$ on PrEP to verify that the states with the largest gay male population would see the largest expansion in PrEP take-up. The notation is the same as in equation III.2, where $PrEP_{st}$ is now the dependent variable. Specifically, we estimate:

$$PrEP_{st} = \sum_{\substack{t=2008 \\ t \neq 2011}}^{2017} \alpha_t \mathbb{1}(Year = t) * \%MaleSSP_s + \gamma X_{st} + \mu_s + \tau_t + \epsilon_{st} \quad (III.3)$$

Our identifying assumption is similar to one mentioned in the previous specification. The assumption is that states with a larger gay population would have similar trends in male STDs as those with a smaller gay population in the absence of PrEP. While this assumption is untestable, we can examine how STDs were evolving before the differential treatment of states, which will be seen by plotting β_{2008} , β_{2009} , and β_{2010} in the results section. Our identifying variation, the percentage of male same-sex partnerships by state, is presented in Figure III.3.

5 Results

5.1 PrEP Rate Specifications

Table III.2 reports the results from estimating equation III.1 separately by STDs. The first column for a STD gives the results from equation III.1, and the second column gives the results using state-specific linear time trends. The equations are limited to male STDs and PrEP rate. All specifications include state by year controls as well as state and year fixed effects and are weighted by population. Standard errors are clustered at the state level.

In column (a) for each STD, we find significant effects for the male PrEP rate on male chlamy-

dia, gonorrhea, and HIV. We estimate that each additional male user of PrEP significantly increases the incidences of male chlamydia and gonorrhea by 0.431 and 0.337, respectively, and reduces HIV diagnoses by 0.119. However, HIV diagnoses are likely downwardly biased given the discussion in the Identification Strategy Section.

Given that we are concerned about differential trends in STDs, we also control for state-specific linear time trends. When controlling for these time trends, we estimate that each additional male user of PrEP increases the incidences of male chlamydia, gonorrhea, and syphilis by 0.575, 0.610, and 0.032 cases, respectively. We also find that one male PrEP user decreases male HIV diagnoses by 0.095 cases, but again, the estimate on HIV may be downwardly biased, overstating the true effect.

We present the results for the expanded sample going back to 2000 for chlamydia, gonorrhea, and syphilis infections in Table III.3. We find a similar pattern to Table III.2 where using the state-specific linear time trends makes our estimates of PrEP on STDs larger. Without the time trends, we find a significant effect of one additional male PrEP user leading to 0.821 additional male cases of chlamydia, 0.533 additional male cases of gonorrhea and 0.0408 additional male cases of syphilis. With time trends, we find a significant effect of one additional male PrEP user leading to 1.159 additional male cases of chlamydia, 0.944 additional male cases of gonorrhea and 0.0675 additional male cases of syphilis. Our results may be larger in this expanded sample if STDs were lower in 2000 to 2007 and were trending upwards for states that had higher PrEP adoption.

Controlling for state-specific linear time trends should help control for differential trends in STDs. We examine how different states were trending in STDs before the introduction of PrEP in our next section.

5.2 Intensity of Rollout from Variation in Gay Population in 2000

We examine how trends in STDs were evolving based in pre-treatment variation before the national approval of PrEP. Specifically, we use the percentage of the male same-sex partnership in a state in the 2000 Census as a proxy for pre-treatment variation in the gay male distribution. States

with a larger gay population before PrEP would be more likely have more adopters of PrEP. Our specifications to estimate these effects are given in equations III.2 and III.3.

In Figure III.5, we plot the α_t from equation III.3 to see how states with larger gay populations adopted PrEP at a faster rate. The interpretation of α_{2017} is the predicted increase in PrEP usage in 2017 relative to 2011 based on a 1 percentage point increase in the percent of male same-sex partnerships. It is clear that states with larger gay populations are more likely to adopt PrEP.

Next, we plot the β_t s in equation III.3 in Figure III.6, which we estimate for male STD rates. The interpretation on a coefficient like β_{2017} is the increase in male STDs in 2017 relative to 2011 for a state based on a 1 percentage point increase in the percent of male same-sex partnerships. We plot male STD rates with 2011 as the comparison year. For chlamydia, gonorrhea, and syphilis, we find that male STD rates tracked extremely similarly in states with large and small gay populations, and after 2012, states with larger gay populations saw male STDs increase. These event studies in Figures III.5 and III.6 support the hypothesis that the PrEP expansion would be largest in states with a larger gay population and those states would also see an increase in male STDs other than HIV. We display these coefficients for β_t and α_t in Table III.4

In Figure III.6, we also estimate how trends were evolving for male HIV diagnoses. We see that the HIV diagnoses for men were both downwardly trending before the introduction with little change afterwards. As discussed before, these differential trends in HIV diagnoses would make estimating an effect on HIV difficult. As such, we are not confident in estimating the effect of PrEP on HIV.

We also present the estimated β_t s for the expanded sample back to 2000 for chlamydia, gonorrhea, and syphilis in Figure III.7. We present the coefficients for these figures in Table III.5. The figures going back to 2000 look less encouraging for chlamydia and syphilis as states with larger gay populations seem have been differentially increasing their chlamydia and syphilis rates going back to 2000, which would explain why the estimates for chlamydia and syphilis were larger in Table III.3 than in Table III.2. However, the trend for gonorrhea before PrEP is remarkably stable, further providing evidence that PrEP has a causal effect on gonorrhea.

6 Back of the Envelope Calculations and Discussion

6.1 Counterfactual

We conduct a simple counterfactual analysis to examine how STDs would evolve in a counterfactual world without PrEP. We take our estimated effect of PrEP on STDs and subtract the number of additional STDs caused by PrEP from aggregate STD numbers. We use the model using 2008-2017 data as it is our main sample, and the pretrends in STDs makes for more reasonable comparisons than the model using 2000-2017 data. We find in our model with state-specific linear time trends that 1 additional male PrEP user results in 0.575 additional male cases of chlamydia, so for each male PrEP user, we subtract 0.575 cases of male chlamydia and recalculate the rate. The implicit assumption in this analysis is that the marginal effect of an additional PrEP user is constant over time and across states, which could be a strong assumption. However, we believe that this analysis can still be instructive. We plot the new estimates for the STD rate for each year. We present the counterfactual estimates based off of the model using the state-specific linear time trends in III.6. We present the counterfactual estimates for chlamydia, gonorrhea and syphilis in Figure III.8. We omit HIV rates because of our lack of confidence in accurately estimating the correct effect of PrEP on HIV and the subsequent counterfactual.

In the top left quadrant, we present the counterfactual for the aggregate chlamydia rate in the absence of PrEP with the top right quadrant showing the counterfactual for gonorrhea and the bottom left quadrant showing the counterfactual for syphilis. We also give how large of a decline the STD cases would be in the absence of PrEP. We present the percent change from the observed cases in Table III.6.

In the chlamydia figure, our estimates suggest that chlamydia cases would be 9.4% lower in aggregate in the absence of PrEP. For gonorrhea, our estimates suggest that the gonorrhea rate would be 17.9% lower in the absence of PrEP. For syphilis, our estimates suggest that the syphilis rate would be 11.1% lower in the absence of PrEP. We do not perform the counterfactual analysis for HIV diagnoses as the estimates are likely biased away from zero, and the estimate sizes are likely too large.

One potential concern with this analysis is that the results are possibly too large to plausible. The counterfactual changes for chlamydia, gonorrhea, and syphilis are relatively high given that the LGBT population is relatively small compared to the non-LGBT population. However, the amount of people that identify as part of the LGBT population is significantly different from the population that engages in sexual behavior with a member of the same-sex. For instance, about 3.5% of the population identifies with the LGBT title, but about 8% of the population has engaged in same-sex sexual behavior and about 11% have some same-sex sexual attraction (Gates, 2011). These estimates are from surveys though, and people could be underreporting same-sex attraction or behavior given the stigma around same-sex attraction, so while MSM are a small section of the overall population, MSM are a larger group than men with same-sex attraction who would identify as part of the LGBT community.

The CDC reports on their website that the majority of male syphilis cases are from MSM at about 78% in 2018¹⁰. This finding would suggest that our estimates of the effect of PrEP on syphilis are potentially plausible. Gonorrhea is also growing specifically among the MSM community. The CDC shows that the rate of gonorrhea among men and women were relatively equal until 2012 when gonorrhea among men starting to shoot up while the rate for women stayed the same, which the CDC suggests is due to increased infections among MSMs who are less likely to have sexual interaction with women¹¹. The CDC has a STD surveillance network giving them greater information on STD in certain cities, allowing them to identify what share of an STD is attributable to MSM. There is a wide range in the share of gonorrhea attributed to MSM with about 86% of gonorrhea cases in San Francisco attributed to MSM to about 20% in Baltimore. The CDC estimates about 43% of gonorrhea cases are attributable to MSM, 21% are attributable to heterosexual men, and 32% attributable to women in their surveillance cities. The figures for men suggest that about 67% of male cases of gonorrhea are attributable to MSM, suggesting that our estimates are again plausible. The CDC does not present the rate of male chlamydia cases attributable to MSM, making any inference about plausibility of counterfactual estimates difficult.

¹⁰<https://www.cdc.gov/std/stats18/msm.htm>

¹¹<https://www.cdc.gov/std/stats18/gonorrhea.htm>

If the prevalence of chlamydia is the same or slightly smaller among MSM as gonorrhea is then our estimates would be plausible.

Overall, our estimates suggest that a large amount of the STDs in 2017 are potentially attributable to PrEP and the rollout of the new HIV-prevention drug. Importantly, our findings are plausible given how many STDs are attributable to MSM, who are most likely to use PrEP. We find that chlamydia cases would be 9% lower, gonorrhea cases would be 18% lower, and syphilis cases would be 11% lower in the absence of PrEP in 2017, suggesting that PrEP contributes to a sizeable share of current male STDs.

6.2 Estimated Costs and Context

We conduct back of the envelope calculations that estimate the additional costs relative to the benefits associated with additional STDs that occurred due to the introduction of PrEP using STD costs estimates from the medical literature. Ideally, we would want to do a cost-benefit analysis to compare the costs of PrEP – the additional STDs caused from a moral hazard effect – to the benefits of PrEP – the reduced cases of HIV. However, any benefit we estimate will likely be overestimated due to the differential pre-trends in the diagnoses of HIV. In Table III.7, we detail the estimate the costs and the overestimated benefits of PrEP. Given that the benefits are likely overstated, we also estimate what the benefits would need to be for PrEP to be welfare-improving to put the costs of PrEP into the proper context.

The first column gives the lifetime cost to treat the given STD based on Owusu-Edusei Jr et al. (2013) which gives costs in 2010 dollars. The estimate includes the costs of common complications of STDs weighted by the likelihood of said complication. We take the estimated effect of PrEP on STDs for our model using state-specific linear time trends from 2008-2017 in Table III.2 and multiply that effect by the cost for each STD to get an average cost of STD per PrEP user. We then multiply the average cost by the number of male PrEP users. In 2017, there were 94,196 male PrEP users, which we use to get the total cost of PrEP in 2017 with respect to additional STDs caused. For the estimates of HIV, the benefits are given as negative costs. Finally, we sum up the costs for all of the STDs and the non-HIV STDs separately, given that the effects of PrEP on HIV

will be overstated. We find the total cost of PrEP with respect to increased incidences of STDs is \$8.3 million in 2017. If one were to take the effect of PrEP on HIV as the true effect that is not biased, then our findings suggest that in 2017, PrEP had a positive benefit of \$2.7 billion.

However, we cannot confidently estimate the true benefits of HIV reduction, leading to an overestimated benefit. Since we cannot be confident in identifying the reduction of PrEP on HIV due to the differential pre-trends in states with high gay populations to those with low gay populations, we can instead calculate how many cases of HIV that PrEP would need to prevent in order to justify the additional costs of the STDs, which we show in the last column of Table III.7. We divide the total cost for each STD by the lifetime cost of contracting HIV – \$304,500, giving us the number of HIV cases that would need to be prevented to offset the cost of the additional STDs. For PrEP to break-even on the cost of the additional STDs, PrEP would need to prevent 27.2 cases based on our estimate. The 94,196 male PrEP users would need to prevent 28 HIV cases for the benefits of PrEP to outweigh the costs, suggesting a needed prevention rate of 0.03%. Each new PrEP user would need to prevent 0.0003 cases. Our estimated effect size of 1 PrEP user preventing 0.0945 cases of HIV is over 300 times larger than the needed effect size for PrEP on HIV to make PrEP a welfare improving drug. While our estimates are biased to overstate the effect, it is unlikely to overstate the effect by 300 times.

In summation, if we were confident in our estimates of the effect of PrEP on HIV, then our results suggest a benefit of \$2.7 billion in 2017 alone. However, as we have discussed extensively, given the downward trend in HIV diagnoses before the introduction of PrEP, our estimates of PrEP on HIV would be biased downward and overstate any effect. Given the difficulty in estimating the benefits, we conduct a break-even analysis to put the costs of the additional STDs from PrEP into context. We find that only about 28 cases of HIV need to be prevented in 2017 for PrEP to have a net benefit effect with respect to the transmission of STDs.

6.3 The Proliferation of Dating Applications

A potential omitted variable in our analysis is the rise of the dating apps, especially Grindr which is targeted at the male gay population and had a large expansion of users at a similar time as the

adoption of PrEP. If Grindr increased the availability of sex among the gay population, it could lead to an increased number of sexual partners and consequently, increased STD incidences in states with a larger gay population. Given that we identify of the share of the population that is gay, our estimates could be picking up some of the effect of Grindr on STDs and not of PrEP.

Although there is no publicly available data on the adaption of Grindr that would have enabled us to test this hypothesis, it is unlikely that our results are driven by the growth of Grindr given what we saw in our event study plots. Grindr was introduced in 2009 and by 2012, the year of PrEP introduction, it already had about 4 million users¹². If indeed the use of Grindr increased STDs, it should show up as positive coefficients in our event study specification illustrated in Figure III.6, for the years 2009, 2010, 2012, which is not the case. Nonetheless, we cannot rule out the possibility that Grindr growing its user base was responsible for an increase in STDs amongst gay men.

7 Conclusion

PrEP is a potentially life-saving drug for thousands of people and gay men specifically by preventing HIV infections. However, given that people may endogenously respond by having riskier sex, it comes with a potential moral hazard response, leading to higher non-HIV STDs. We conduct the first analysis of PrEP on aggregate STDs, estimating the moral hazard response and what the cost of that response is. Our paper also contributes to the extensive literature in economics on moral hazard and specifically, the literature on HIV treatments and moral hazard. Finally, we also contribute to the epidemiology literature, helping to explain the recent rise in STDs.

We present convincing evidence that PrEP increases male STDs. We estimate that one additional male PrEP user increases male chlamydia infections by 0.55 cases, gonorrhea infections by 0.61 cases, and syphilis infections by 0.03 cases. We have difficulty in estimating the unbiased effect of PrEP on HIV due to differential trends in HIV infections before PrEP. The non-HIV STDs do not have this issue where male STD rates for state with high and gay populations track

¹²<https://techcrunch.com/2012/06/17/grindr-joel-simkhai-announces-4m-users-1m-daily-uniques-and-weighs-in-on-the-skout-disaster/>

extremely parallel, centered around zero before the introduction of PrEP, and afterwards, male STDs increase more in places with a larger gay male population.

We show that the increase in STDs from PrEP accounts for a large share of STDs in the aggregate. We estimate a counterfactual analysis for our findings, showing that chlamydia, gonorrhea, and syphilis cases were 9%, 18%, and 11% higher, respectively, in 2017 compared to a situation where PrEP was not introduced. We then analyze if PrEP is welfare-improving. First, we estimate the costs of the additional STDs, which we estimate at about \$8.3 million in 2017. If one were to take our biased estimate of PrEP on HIV, then we find a net-benefit of \$2.7 billion in 2017. However, given that we believe that the effect of PrEP on HIV is overstated, we instead conduct a break-even analysis where we calculate how many cases of HIV need to be prevented to make PrEP a cost-neutral. We find that the 94,169 additional male PrEP users in 2017 would need to prevent only 28 male cases of HIV, a prevention rate of 0.03%, to be net-positive. Our estimated effect of PrEP on HIV is over 300 times the necessary prevention rate for PrEP to be welfare enhancing.

Overall, we present the first estimates on the aggregate costs of PrEP from a moral hazard standpoint. While we are more confident in estimating costs than benefits, our findings suggest that PrEP would need to prevent so few cases of HIV that it is almost assuredly welfare improving.

Tables

Table III.1. Summary Statistics

Variable	2008-2017	2008-2011		2012-2017	
	All States	Low PrEP States	High PrEP States	Low PrEP States	High PrEP States
Male Chlamydia Rate	328	293	298	356	387
Male Gonorrhea Rate	149	122	110	158	184
Male Syphilis Rate	13.22	5.73	12.44	8.87	20.93
Male HIV Diagnosis Rate	25.75	17.11	34.33	16.65	28.58
Male PrEP Rate	19.36	0.00	0.00	11.04	48.47
Percent Male Same-Sex Partners in 2000	0.56	0.39	0.69	0.39	0.69
Percent White	0.614	0.740	0.546	0.724	0.518
Percent Hispanic	0.103	0.033	0.125	0.040	0.134
Percent Black	0.119	0.134	0.107	0.133	0.106
Percent Asian	0.049	0.013	0.071	0.016	0.078
Unemployment rate	0.070	0.073	0.091	0.054	0.065
Poverty Rate	0.140	0.150	0.141	0.151	0.134
SNAP Recipients	1766285	401184.5	2033476	454479.5	2807407
Natural Log of GSP (in Millions)	13.13	11.42	13.63	11.58	13.81

Note: The table provides summary statistics for relevant variables. We break the sample up in different ways with the first column showing the different male STD rates and descriptive statistics for our full sample. The rate for PrEP adoption and STD proliferation is given as cases per 100,000. We also split states by if they were a high PrEP adoption or low PrEP adoption state, which we define as being the highest or lowest quartile of PrEP adoption in 2017, respectively. We split that sample into before and after the introduction of PrEP as well. The summary statistics are weighted by population.

Table III.2. PrEP on Male STDs: 2008-2017

VARIABLES	Chlamydia		Gonorrhea		Syphilis		HIV	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
PrEP Rate	0.431*** (0.131)	0.575*** (0.110)	0.337** (0.136)	0.610*** (0.121)	0.0148 (0.00927)	0.0315*** (0.0108)	-0.119*** (0.0305)	-0.0945*** (0.0281)
State-Specific Linear Time Trends		X		X		X		X
Dependent Var. Mean	323	323	133	133	10.6	10.6	21.7	21.7
Observations	510	510	510	510	503	503	510	510
R-squared	0.949	1.000	0.936	1.000	0.914	1.000	0.954	1.000

Note: The table provides the results from estimating equation III.1 where the outcomes variables are the male chlamydia, gonorrhea, syphilis, and HIV rate. For each STD, columns (a) are the results of difference-in-difference equation III.1 estimated for male STDs and columns (b) give the results using the state-specific linear time trends. All specifications include year and state fixed effects and are population weighted. Controls include the racial share of the state population, log state GDP, the unemployment rate, SNAP recipients, and the poverty rate. Standard errors are clustered at the state level are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table III.3. PrEP on Male STDs: 2000-2017

VARIABLES	Chlamydia		Gonorrhea		Syphilis	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
PrEP Rate	0.821*** (0.155)	1.159*** (0.235)	0.533*** (0.0919)	0.944*** (0.111)	0.0408*** (0.0117)	0.0675*** (0.0165)
State-Specific Linear Time Trends		X		X		X
Dependent Var. Mean	248	248	128	128	7.9	7.9
Observations	918	918	918	918	911	911
R-squared	0.890	1.000	0.890	1.000	0.877	1.000

Note: The table provides the results from estimating equation III.1 where the outcomes variables are the male chlamydia, gonorrhea, and syphilis rate. HIV is omitted as the data on HIV infections stops in 2008. For each STD, columns (a) are the results of difference-in-difference equation III.1 estimated for male STDs and columns (b) give the results using the state-specific linear time trends. All specifications include year and state fixed effects and are population weighted. Controls include the racial share of the state population, log state GDP, the unemployment rate, SNAP recipients, and the poverty rate. Standard errors are clustered at the state level are in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table III.4. Pre-Treatment Variation in Difference-in-Differences for Male STDs: 2008-2017

VARIABLES	(1) Chlamydia	(2) Gonorrhea	(3) Syphilis	(4) HIV	(5) PrEP
β_{2008}	47.50*** (16.75)	-4.661 (19.92)	2.000 (2.760)	29.43*** (6.145)	-3.767 (7.453)
β_{2009}	35.51** (16.70)	4.258 (13.96)	-0.169 (2.197)	13.58*** (3.345)	0.731 (5.556)
β_{2010}	-17.20 (14.73)	-15.10 (10.22)	-2.387** (1.076)	8.822*** (3.215)	-0.109 (2.426)
β_{2011} (Omitted)	-	-	-	-	-
β_{2012}	-10.74 (10.64)	-0.00467 (7.420)	0.00868 (1.370)	-5.548*** (1.678)	11.00* (5.802)
β_{2013}	-5.116 (13.03)	20.00* (11.07)	-0.0586 (1.760)	-13.39*** (1.599)	20.10* (10.70)
β_{2014}	-50.97 (36.11)	1.250 (40.31)	-2.236 (4.483)	-14.10*** (3.163)	44.58*** (8.345)
β_{2015}	60.82*** (14.25)	61.04** (26.33)	-1.281 (7.126)	-18.79*** (4.336)	99.18*** (9.313)
β_{2016}	71.04*** (19.80)	89.92*** (20.34)	4.364 (6.297)	-21.61*** (4.296)	141.0*** (15.82)
β_{2017}	162.8*** (21.24)	144.3*** (19.45)	12.59*** (3.957)	-24.03*** (3.869)	165.3*** (20.52)
Observations	510	510	503	510	510
R-squared	0.958	0.945	0.923	0.971	0.891

Note: This table gives the relevant coefficients from equations III.2 and III.3 estimated for male STD and PrEP rate. We report the β_t s from equation III.2, which gives the additional cases per 100,000 of an STD that occurs in year t with a 1 p.p. increase in the percent of male same-sex partnerships in the 2000 Census. For context, the difference in the percentage of male same-sex partnerships between New York and North Dakota is about 0.5 p.p., so halving the coefficient values reported is the predicted difference in STDs between New York and North Dakota from PrEP expansion. We also report the α_t s from equation III.3 to show how PrEP expansion is affected by the gay population in a state. Standard errors are clustered at the state level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table III.5. Pre-Treatment Variation in Difference-in-Differences for Male STDs: 2000-2017

VARIABLES	(1) Chlamydia	(2) Gonorrhea	(3) Syphilis
β_{2000}	-123.2** (49.72)	-12.93 (28.38)	-14.22*** (1.767)
β_{2001}	-120.6** (45.30)	4.697 (28.09)	-11.96*** (1.463)
β_{2002}	-124.2*** (45.77)	0.0713 (20.15)	-8.111*** (2.316)
β_{2003}	-120.8** (46.05)	-8.000 (17.64)	-6.323* (3.612)
β_{2004}	-116.1** (46.81)	-12.61 (17.89)	-4.126 (3.072)
β_{2005}	-107.9** (43.99)	-25.25 (19.14)	-2.587 (1.738)
β_{2006}	-98.86** (42.87)	-31.25* (18.36)	-2.471 (1.991)
β_{2007}	-16.96 (27.83)	-21.33 (18.23)	4.393** (1.947)
β_{2008}	37.49* (19.59)	-16.68 (22.92)	1.273 (1.719)
β_{2009}	21.66 (20.69)	-10.49 (14.94)	-1.084 (1.967)
β_{2010}	-21.31 (15.23)	-19.69 (12.34)	-2.885*** (0.785)
Continued on next page			

Table III.5 – continued from previous page

Variable	Chlamydia	Gonorrhea	Syphilis
β_{2011} (Omitted)	-	-	-
β_{2012}	-7.448 (9.877)	8.852 (10.27)	1.582 (1.649)
β_{2013}	-3.427 (15.64)	28.32* (16.65)	1.579 (3.019)
β_{2014}	-41.13 (44.26)	11.46 (47.07)	0.287 (5.466)
β_{2015}	78.26*** (19.52)	77.37** (36.38)	2.311 (8.792)
β_{2016}	88.06*** (29.39)	106.3*** (31.93)	8.556 (8.455)
β_{2017}	183.6*** (17.80)	163.5*** (20.34)	17.02** (6.424)
Observations	918	918	911
R-squared	0.942	0.898	0.902

Note: This table gives the relevant coefficient from equations III.2 estimated for the male STD rate for chlamydia, gonorrhea and syphilis going back to 2000. HIV is omitted as the data on HIV infections stops in 2008. We report the β_t s from equation III.2, which gives the additional cases per 100,000 of an STD that occurs in year t with a 1 p.p. increase in the percent of male same-sex partnerships in the 2000 Census. For context, the difference in the percentage of male same-sex partnerships between New York and North Dakota is about 0.5 p.p., so halving the coefficient values reported is the predicted difference in STDs between New York and North Dakota from PrEP expansion. Standard errors are clustered at the state level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table III.6. Counterfactual Analysis

STDs	Observed Cases	Counterfactual Cases	Percent Change
Chlamydia	576,857	522,722.2	-9.4%
Gonorrhea	321,857	264,375.3	-17.9%
Syphilis	26,869	23,899.3	-11.1%

Note: This table reports an estimated a counterfactual of what the cases in STDs would be in the absence of PrEP in 2017 based on our estimates using state-specific linear time trends reported in Table III.2. We only report counterfactuals for chlamydia, gonorrhea, and syphilis as we are not confident that we are accurately capturing the effect of PrEP on HIV. We present the observed cases in 2017 in the first column with the counterfactual cases and the percent change. To estimate the counterfactual, we take the effect of an additional male PrEP user on an STD and multiply it by the number of male PrEP users in 2017 and subtract that number from the observed number of cases. This analysis implicitly assumes that effects are constant across time and states.

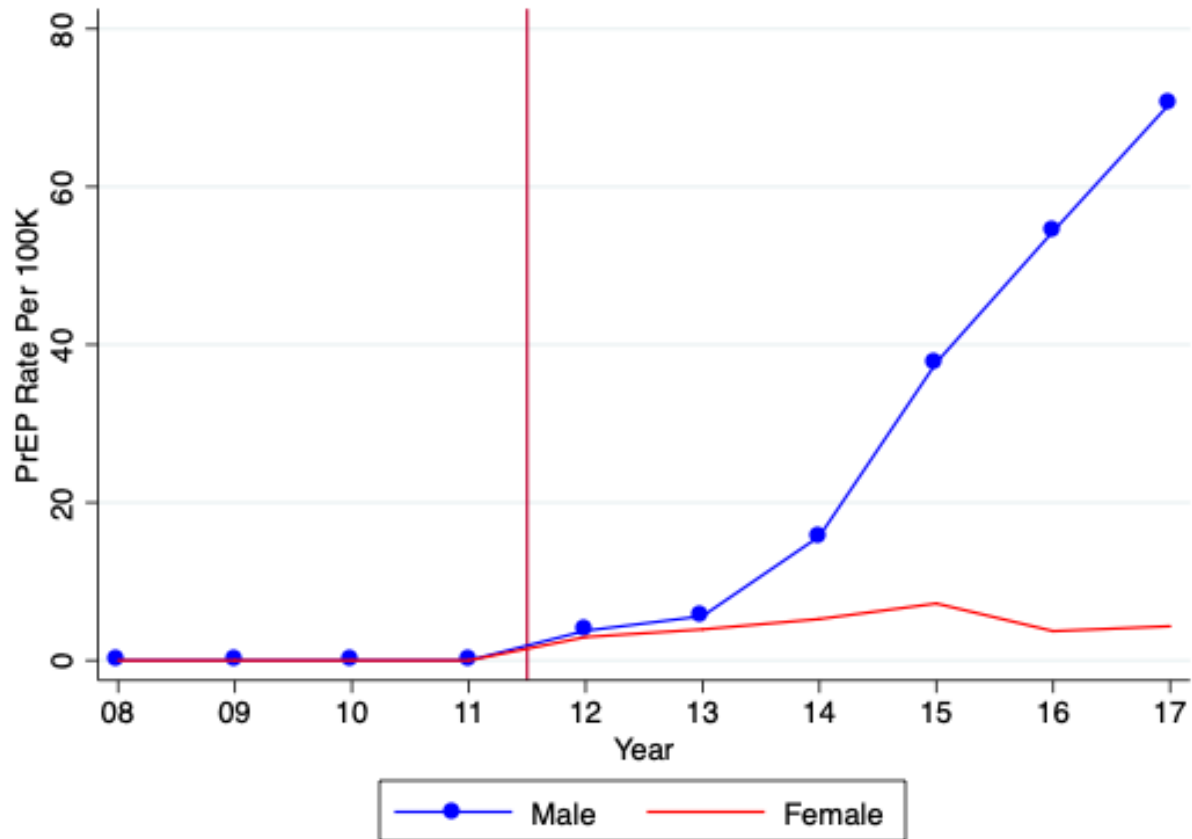
Table III.7. Costs in Context

STDs	Cost to Treat	Avg Cost Per PrEP User	Total Cost	Prevented HIV Cases to Offset
Chlamydia	\$30	\$17.25	\$1,624,881	5.3
Gonorrhea	\$79	\$48.19	\$4,539,305	14.9
Syphilis	\$709	\$22.33	\$2,103,726	6.9
HIV	\$304,500	-\$28,775.25	-\$2,710,513,449	-
Total (Non-HIV STDs)	\$818	\$88	\$8,267,913	27.2
Total	-	-\$28,687	-\$2,702,245,536	-

Note: This table puts the costs of PrEP into context. It reports an estimated naïve cost-benefit analysis comparing the costs of PrEP – the additional STDs – to the benefit of PrEP – the reduction in HIV as well as a break-even analysis. Given that trends in HIV diagnoses were downwardly trending before the introduction of PrEP, our estimated benefits will overstate the true effect. We present the lifetime costs of treating different STDs in 2010 dollars using Owusu-Edusei Jr et al. (2013). We give the average estimated costs using state-specific linear time trends presented in Table III.2 by taking the effect of an additional male PrEP user on male STDs and multiply it by cost of treatment. To get total cost, we take the average cost per PrEP user and multiply it by the how many male PrEP users there are in 2017 – 94,169. Benefits are given as negative costs. We give the total cost for all STDs and non-HIV STDs separately given our lack of confidence in the HIV results. We then estimate how many cases of HIV would need to be prevented to make the policy neutral on net, which we do by dividing the total cost of treating an STD by lifetime cost of treating one HIV case, \$304,500.

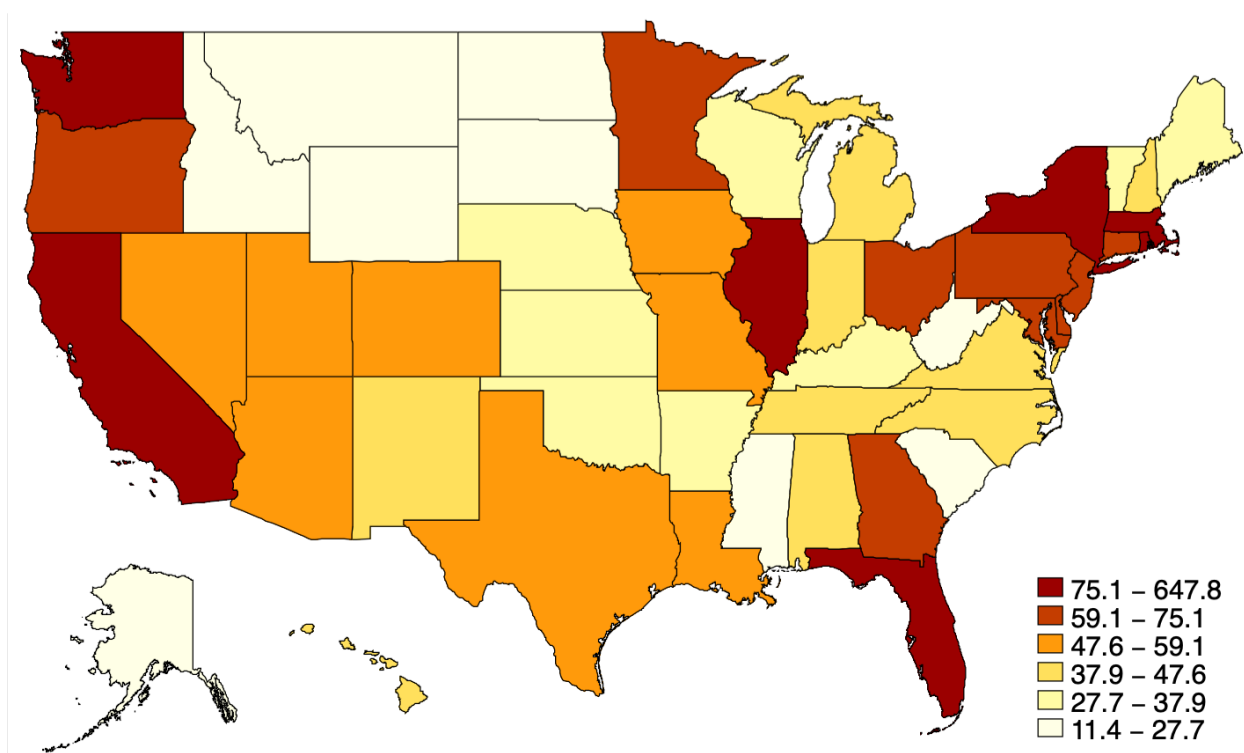
Figures

Figure III.1. Male and Female PrEP Use Over Time



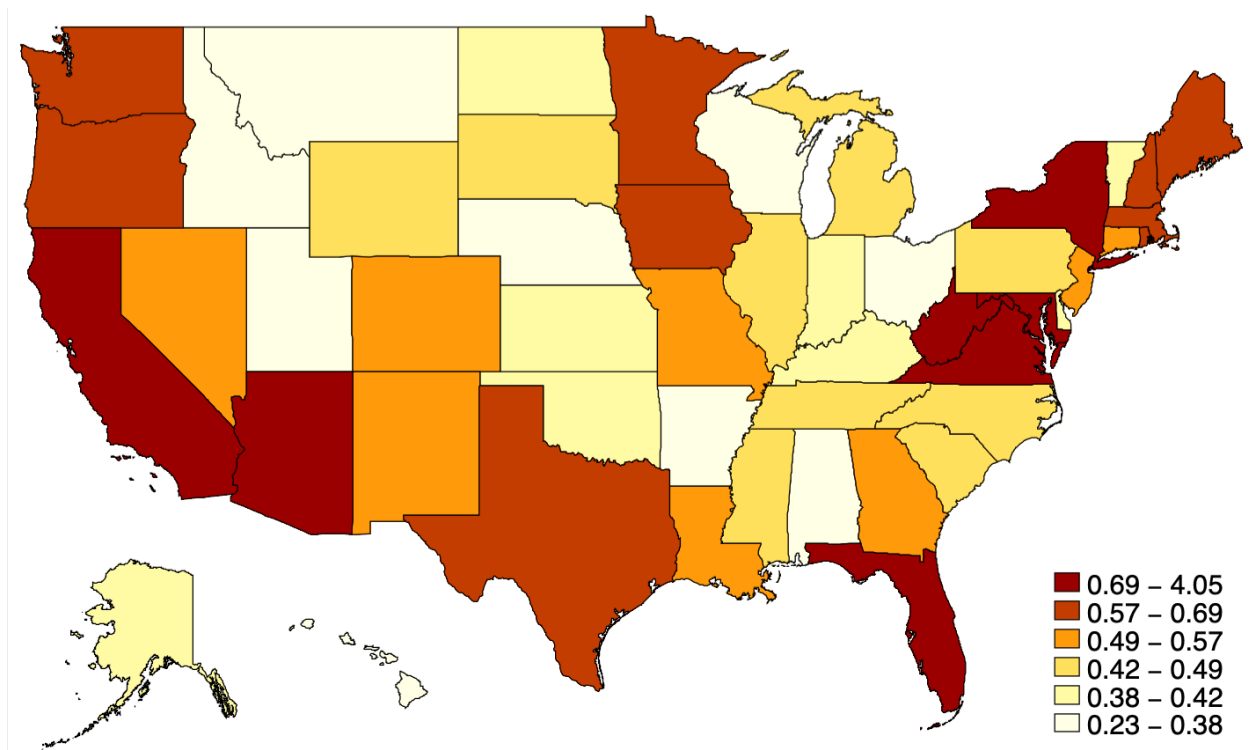
Note: This figure presents the descriptive statistics on the different rollout of PrEP between men and women between 2008 and 2017. The rate is given as users per 100,000. PrEP data is from AIDSvu.

Figure III.2. PrEP Use Across States



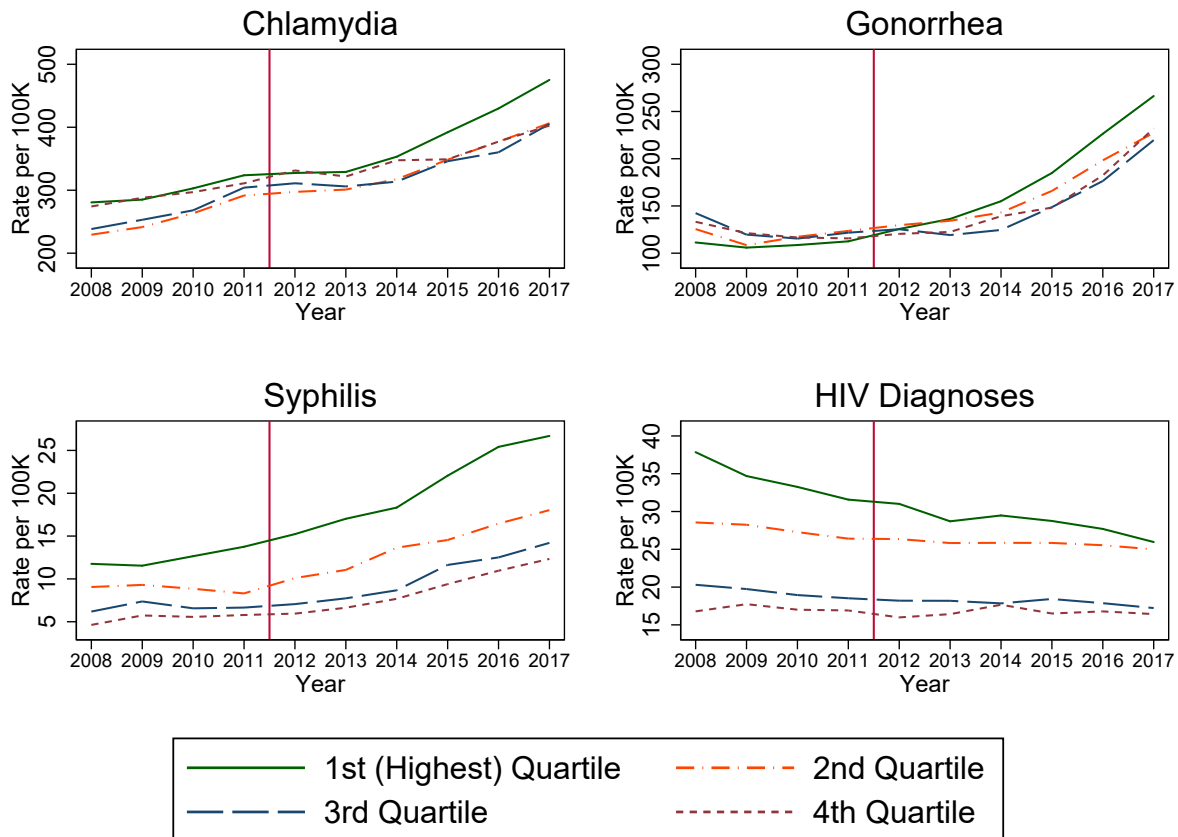
Note: This figure presents the descriptive statistics on the different rollout of PrEP for men in 2017 in different states broken up into sextiles. The rate is given as users per 100,000. PrEP data is from AIDSvu.

Figure III.3. Gay Male Population Across States in 2000 Census



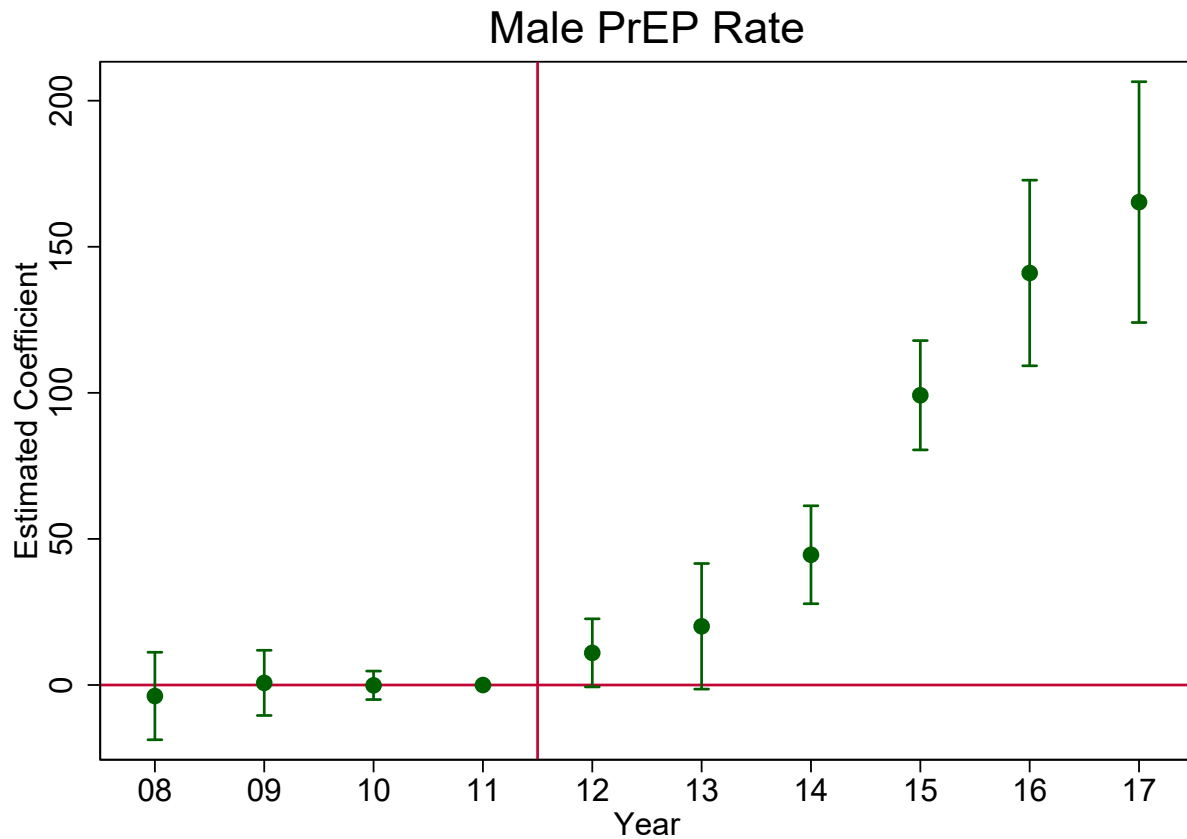
Note: This figure presents the distribution of gay men across states in 2000 as defined as the percentage of all partnerships in a given state based on household composition in the 2000 Census. We break the states up into sextiles based on the gay male population.

Figure III.4. Evolution of Male STD Rates by Quartile of PrEP Take-up



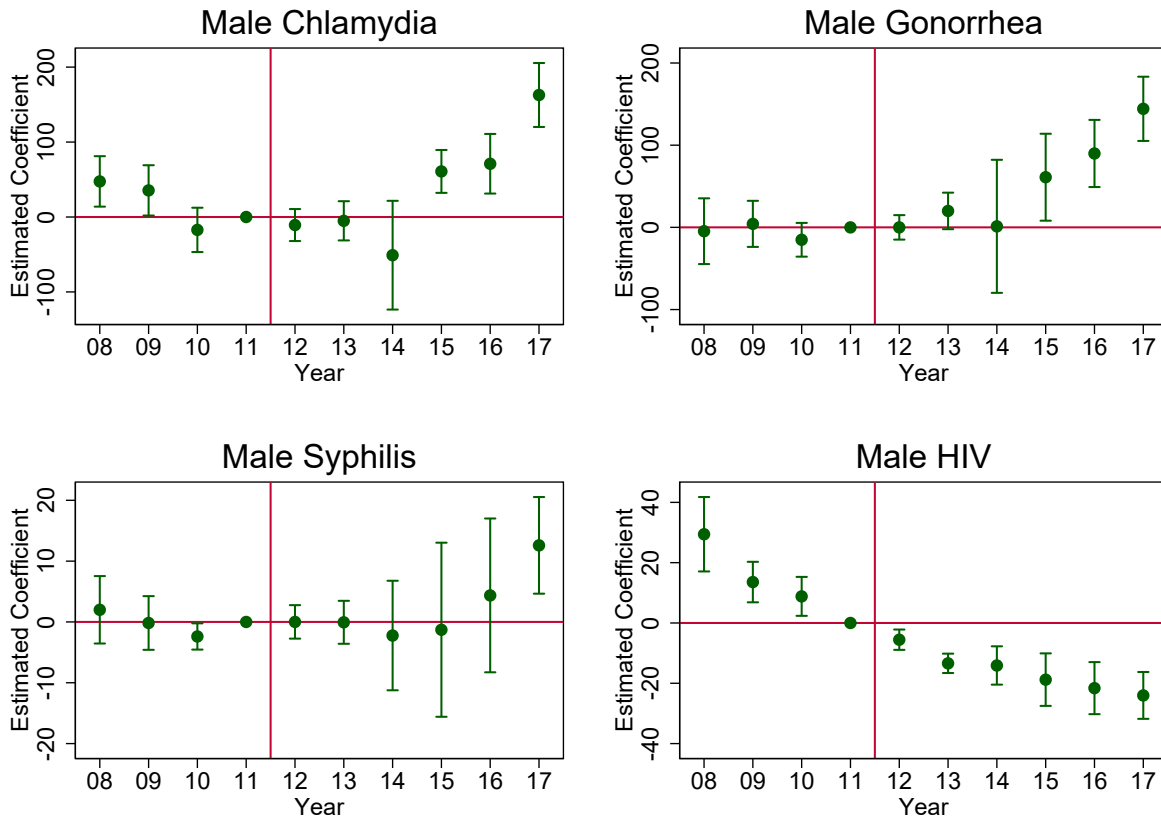
Note: This figure plots the descriptive male STD rates per 100,000 between 2008 and 2017 for states with different PrEP take-up. We split the states into quartiles based on the PrEP adoption in 2017. We present chlamydia, gonorrhea, syphilis, and HIV rates separately. Many of the states look extremely similar with respect to non-HIV STDs before the introduction of PrEP with high adoption states gaining more STDs after the introduction of PrEP.

Figure III.5. Event Study for Pre-Treatment Variation in Difference-in-Differences for Male PrEP Adoption



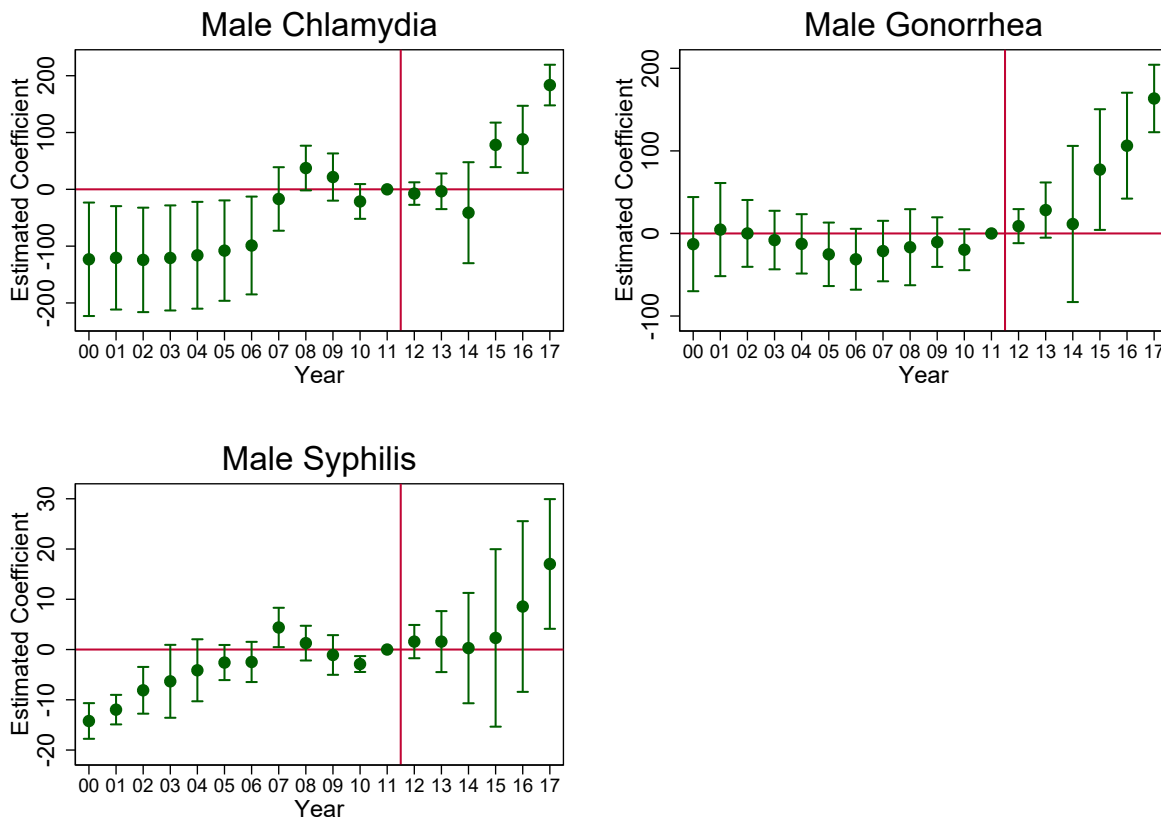
Note: This figure plots the event-study estimates from equation III.3 where we plot the α_t s. The coefficients give the additional PrEP users per 100,000 that occurs in year t with a 1 p.p. increase in the percent of male same-sex partnerships in the 2000 Census. For context, the difference in the percentage of male same-sex partnerships between New York and North Dakota is about 0.5 p.p., so halving the coefficient values reported is the predicted difference in PrEP expansion between New York and North Dakota. The bands represent the 95% confidence interval. Standard errors are clustered at the state level.

Figure III.6. Event Study for Pre-Treatment Variation in Difference-in-Differences for Male STD Rates: 2008-2017



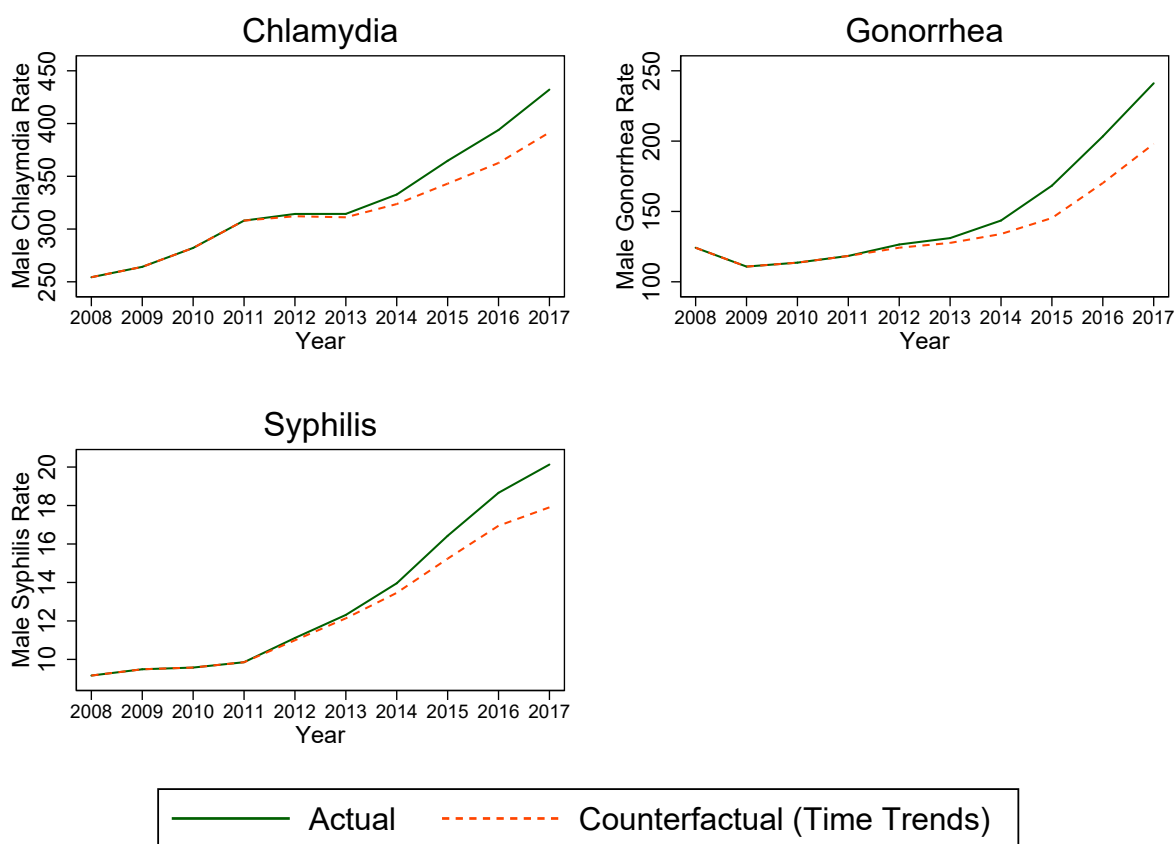
Note: This figure plots the event-study estimates from equation III.2 where we plot the β_t s going back to 2008. The coefficients give the additional STDs per 100,000 that occurs in year t with a 1 p.p. increase in the percent of male same-sex partnerships in the 2000 Census. For context, the difference in the percentage of male same-sex partnerships between New York and North Dakota is about 0.5 p.p., so halving the coefficient values reported is the predicted difference in PrEP expansion between New York and North Dakota. The bands represent the 95% confidence interval. Standard errors are clustered at the state level.

Figure III.7. Event Study for Pre-Treatment Variation in Difference-in-Differences for Male STD Rates: 2000-2017



Note: This figure plots the event-study estimates from equation III.2 where we plot the β_t s going back to 2000. HIV is omitted as the data on HIV infections stops in 2008. The coefficients give the additional STDs per 100,000 that occurs in year t with a 1 p.p. increase in the percent of male same-sex partnerships in the 2000 Census. For context, the difference in the percentage of male same-sex partnerships between New York and North Dakota is about 0.5 p.p., so halving the coefficient values reported is the predicted difference in PrEP expansion between New York and North Dakota. The bands represent the 95% confidence interval. Standard errors are clustered at the state level.

Figure III.8. Counterfactual STD Development in the Absence of PrEP



Note: These figures plot how the aggregate STD rate would have developed in the absence of PrEP using our estimates given in the state-specific linear time trends model in Table III.2. We present the counterfactual cases in Table III.6

Appendix A. Tables

Table A1. Black Student to Black Teacher Sorting

VARIABLES	(1) Black Teacher	(2) Black Teacher	(3) Black Teacher	(4) Black Teacher
Black Student	0.152*** (0.0117)	0.00801*** (0.000747)	0.00797*** (0.000745)	0.00376*** (0.000468)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	8,960,036	8,960,036	8,960,036	8,958,612
R-squared	0.039	0.232	0.232	0.620

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Black students and Black teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A2. Hispanic Student to Hispanic Teacher Sorting

VARIABLES	(1) Hispanic Teacher	(2) Hispanic Teacher	(3) Hispanic Teacher	(4) Hispanic Teacher
Hispanic Student	0.218*** (0.0143)	0.00291*** (0.000430)	0.00281*** (0.000428)	0.00122*** (0.000294)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	8,960,036	8,960,036	8,960,036	8,958,612
R-squared	0.039	0.232	0.232	0.620

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Hispanic students and Hispanic teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A3. Asian Student to Asian Teacher Sorting

VARIABLES	(1) Asian Teacher	(2) Asian Teacher	(3) Asian Teacher	(4) Asian Teacher
Asian Student	0.0266*** (0.00343)	0.00361*** (0.00109)	0.00360*** (0.00109)	0.00144** (0.000675)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	8,960,036	8,960,036	8,960,036	8,958,612
R-squared	0.039	0.232	0.232	0.620

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Asian students and Asian teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A4. White Student to White Teacher Sorting

VARIABLES	(1) White Teacher	(2) White Teacher	(3) White Teacher	(4) White Teacher
White Student	0.261*** (0.0126)	0.00455*** (0.000589)	0.00437*** (0.000587)	0.00193*** (0.000368)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	8,960,036	8,960,036	8,960,036	8,958,612
R-squared	0.039	0.232	0.232	0.620

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that White students and White teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A5. Covariates of Black Students that Predict Black Teachers

VARIABLES	(1) Black Teacher	(2) Black Teacher	(3) Black Teacher	(4) Black Teacher
Gifted	0.0381*** (0.0101)	-0.0151*** (0.00291)	-0.0150*** (0.00290)	-0.00586** (0.00232)
Free/Reduced Lunch	0.0531*** (0.00689)	0.00190** (0.000814)	0.00179** (0.000807)	0.00105* (0.000595)
8th Grade Reading Z-Score	0.00880*** (0.00250)	0.00283*** (0.00104)	0.00263** (0.00104)	-0.00133** (0.000598)
8th Grade Math Z-Score	-0.00662*** (0.00254)	0.00460*** (0.000801)	0.00475*** (0.000825)	0.00244*** (0.000625)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	1,130,802	1,130,781	1,130,781	1,104,843
R-squared	0.013	0.280	0.280	0.654

Note: Table give regressions estimated separately using varying fixed effects to estimate what covariates of Black students predict having Black teachers matching along racial lines. Regressions are limited to Black students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A6. Covariates of Hispanic Students that Predict Hispanic Teachers

VARIABLES	(1) Hispanic Teacher	(2) Hispanic Teacher	(3) Hispanic Teacher	(4) Hispanic Teacher
Gifted	0.100*** (0.0129)	-0.00901*** (0.00136)	-0.00888*** (0.00136)	-0.00362*** (0.000999)
Free/Reduced Lunch	0.0926*** (0.0121)	0.00243*** (0.000588)	0.00244*** (0.000588)	0.00153*** (0.000395)
8th Grade Reading Z-Score	-0.0316*** (0.00409)	-0.00246*** (0.000675)	-0.00318*** (0.000673)	-0.00140*** (0.000414)
8th Grade Math Z-Score	0.0335*** (0.00515)	-0.000222 (0.000649)	0.000103 (0.000651)	-0.000410 (0.000405)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	4,415,867	4,415,856	4,415,856	4,400,327
R-squared	0.017	0.461	0.461	0.732

Note: This table gives regressions estimated separately using varying fixed effects to estimate what covariates of Hispanic students predict having Hispanic teachers matching along racial lines. Regressions are limited to Hispanic students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A7. Covariates of Asian Students that Predict Asian Teachers

VARIABLES	(1) Asian Teacher	(2) Asian Teacher	(3) Asian Teacher	(4) Asian Teacher
Gifted	0.00869 (0.00673)	0.00135 (0.00287)	0.00174 (0.00290)	0.000104 (0.00142)
Free/Reduced Lunch	0.00525 (0.00358)	-0.000954 (0.000851)	-0.00106 (0.000843)	-0.00121* (0.000702)
8th Grade Reading Z-Score	-0.00210 (0.00144)	-0.00110 (0.000983)	-0.00265** (0.00110)	-0.00167** (0.000793)
8th Grade Math Z-Score	0.000142 (0.00191)	-0.00119 (0.000995)	-0.000311 (0.00109)	0.000430 (0.000647)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	400,796	400,785	400,785	379,993
R-squared	0.001	0.062	0.062	0.579

Note: This table gives regressions estimated separately using varying fixed effects to estimate what covariates of Asian students predict having Asian teachers matching along racial lines. Regressions are limited to Asian students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A8. Covariates of White Students that Predict White Teachers

VARIABLES	(1) White Teacher	(2) White Teacher	(3) White Teacher	(4) White Teacher
Gifted	-0.0112*** (0.00363)	0.0105*** (0.00131)	0.0102*** (0.00130)	0.00525*** (0.00103)
Free/Reduced Lunch	-0.0163*** (0.00280)	-0.00363*** (0.000565)	-0.00356*** (0.000564)	-0.00145*** (0.000398)
8th Grade Reading Z-Score	-0.00439*** (0.00116)	-0.00114* (0.000615)	0.000206 (0.000605)	0.00118*** (0.000423)
8th Grade Math Z-Score	0.00674*** (0.00156)	-0.000385 (0.000568)	-0.000998* (0.000565)	-0.000623 (0.000464)
HS FE		X	X	X
HS, Year FE			X	X
HS by Year by Course FE				X
Observations	2,965,012	2,964,999	2,964,999	2,949,041
R-squared	0.002	0.129	0.129	0.562

Note: This table gives regressions estimated separately using varying fixed effects to estimate what covariates of White students predict having White teachers matching along racial lines. Regressions are limited to White students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A9. Dosage – Black Student to Black Teacher Sorting

VARIABLES	(1) Black Teacher	(2) Black Teacher	(3) Black Teacher	(4) Black Teacher
Black Student	0.669*** (0.0557)	0.0239*** (0.00447)	0.0133*** (0.00309)	0.0142*** (0.00306)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	562,255	562,246	514,501	506,034
R-squared	0.091	0.627	0.749	0.773

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Black students and Black teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A10. Dosage – Hispanic Student to Hispanic Teacher Sorting

VARIABLES	(1) Hispanic Teacher	(2) Hispanic Teacher	(3) Hispanic Teacher	(4) Hispanic Teacher
Hispanic Student	0.973*** (0.0638)	0.0119*** (0.00331)	0.00619*** (0.00237)	0.00547** (0.00231)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	562,255	562,246	514,501	506,034
R-squared	0.172	0.801	0.876	0.889

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Hispanic students and Hispanic teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A11. Dosage – Asian Student to Asian Teacher Sorting

VARIABLES	(1) Asian Teacher	(2) Asian Teacher	(3) Asian Teacher	(4) Asian Teacher
Asian Student	0.0905*** (0.0143)	0.0166*** (0.00532)	0.00917*** (0.00336)	0.00798** (0.00334)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	562,255	562,246	514,501	506,034
R-squared	0.009	0.279	0.472	0.518

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that Asian students and Asian teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A12. Dosage – White Student to White Teacher Sorting

VARIABLES	(1) White Teacher	(2) White Teacher	(3) White Teacher	(4) White Teacher
White Student	1.144*** (0.0524)	0.0403*** (0.00492)	0.00895*** (0.00286)	0.00763*** (0.00283)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	562,255	562,246	514,501	506,034
R-squared	0.171	0.668	0.835	0.851

Note: Table gives the regressions estimated separately using varying fixed effects to estimate the likelihood that White students and White teachers are matched. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A13. Dosage – Covariates of Black Students that Predict Black Teachers

VARIABLES	(1) Black Teacher	(2) Black Teacher	(3) Black Teacher	(4) Black Teacher
Gifted	0.198*** (0.0565)	-0.0415* (0.0241)	0.00584 (0.0164)	0.00616 (0.0154)
Free/Reduced Lunch	0.293*** (0.0377)	0.0163*** (0.00591)	0.00909* (0.00552)	0.00611 (0.00563)
8th Grade Reading Z-Score	0.0255* (0.0150)	-0.0115 (0.00752)	0.00118 (0.00621)	-0.00104 (0.00621)
8th Grade Math Z-Score	-0.0282** (0.0134)	0.0144** (0.00681)	0.00178 (0.00643)	0.000954 (0.00610)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	73,062	72,910	59,580	57,117
R-squared	0.033	0.680	0.797	0.819

Note: Regressions estimated separately using varying fixed effects to estimate what covariates of Black students predict having Black teachers matching along racial lines. Regressions are limited to Black students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A14. Dosage – Covariates of Hispanic Students that Predict Hispanic Teachers

VARIABLES	(1) Hispanic Teacher	(2) Hispanic Teacher	(3) Hispanic Teacher	(4) Hispanic Teacher
Gifted	0.510*** (0.0659)	-0.0172 (0.0130)	-0.00639 (0.00789)	-0.00808 (0.00776)
Free/Reduced Lunch	0.418*** (0.0538)	0.00818* (0.00431)	0.00724** (0.00362)	0.00634* (0.00351)
8th Grade Reading Z-Score	-0.163*** (0.0204)	-0.0376*** (0.00673)	0.00445 (0.00557)	-0.00298 (0.00396)
8th Grade Math Z-Score	0.144*** (0.0250)	-0.0132** (0.00633)	-0.00381 (0.00626)	0.00189 (0.00414)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	270,465	270,429	238,125	232,276
R-squared	0.028	0.807	0.889	0.901

Note: Regressions estimated separately using varying fixed effects to estimate what covariates of Hispanic students predict having Hispanic teachers matching along racial lines. Regressions are limited to Hispanic students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A15. Dosage – Covariates of Asian Students that Predict Asian Teachers

VARIABLES	(1) Asian Teacher	(2) Asian Teacher	(3) Asian Teacher	(4) Asian Teacher
Gifted	0.0237 (0.0331)	-0.00197 (0.0160)	0.00646 (0.0111)	0.00480 (0.0127)
Free/Reduced Lunch	0.00669 (0.0150)	-0.0135* (0.00737)	-0.0140** (0.00652)	-0.0121* (0.00700)
8th Grade Reading Z-Score	-0.00579 (0.00866)	0.000577 (0.00815)	0.00918 (0.0101)	0.00140 (0.00854)
8th Grade Math Z-Score	0.00538 (0.0119)	-0.00528 (0.00987)	0.000744 (0.00896)	0.000631 (0.00778)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	24,274	24,069	18,534	17,438
R-squared	0.001	0.329	0.564	0.616

Note: Regressions estimated separately using varying fixed effects to estimate what covariates of Asian students predict having Asian teachers matching along racial lines. Regressions are limited to Asian students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A16. Dosage – Covariates of White Students that Predict White Teachers

VARIABLES	(1) White Teacher	(2) White Teacher	(3) White Teacher	(4) White Teacher
Gifted	-0.0391* (0.0227)	0.0481*** (0.0129)	0.0186** (0.00842)	0.0207** (0.00832)
Free/Reduced Lunch	0.0179 (0.0169)	-0.0182*** (0.00590)	-0.00265 (0.00379)	-0.00283 (0.00356)
8th Grade Reading Z-Score	-0.0589*** (0.00885)	-0.0442*** (0.00697)	-0.000535 (0.00647)	0.0111*** (0.00410)
8th Grade Math Z-Score	0.00843 (0.0110)	-0.0295*** (0.00661)	0.00699 (0.00625)	-0.000560 (0.00383)
HS FE		X	X	X
Course Set			X	X
Cohort Course Set				X
Observations	191,190	191,115	168,195	163,583
R-squared	0.003	0.427	0.779	0.801

Note: Regressions estimated separately using varying fixed effects to estimate what covariates of White students predict having White teachers matching along racial lines. Regressions are limited to White students. Standard errors are clustered at the school level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

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